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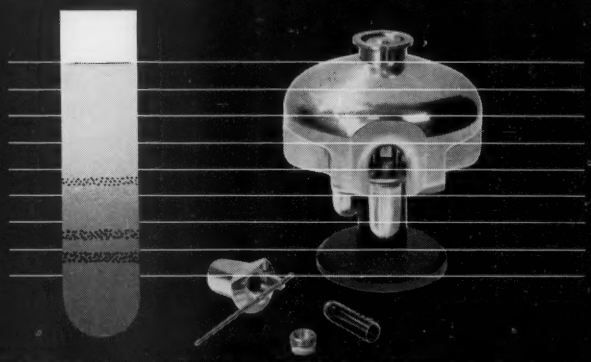

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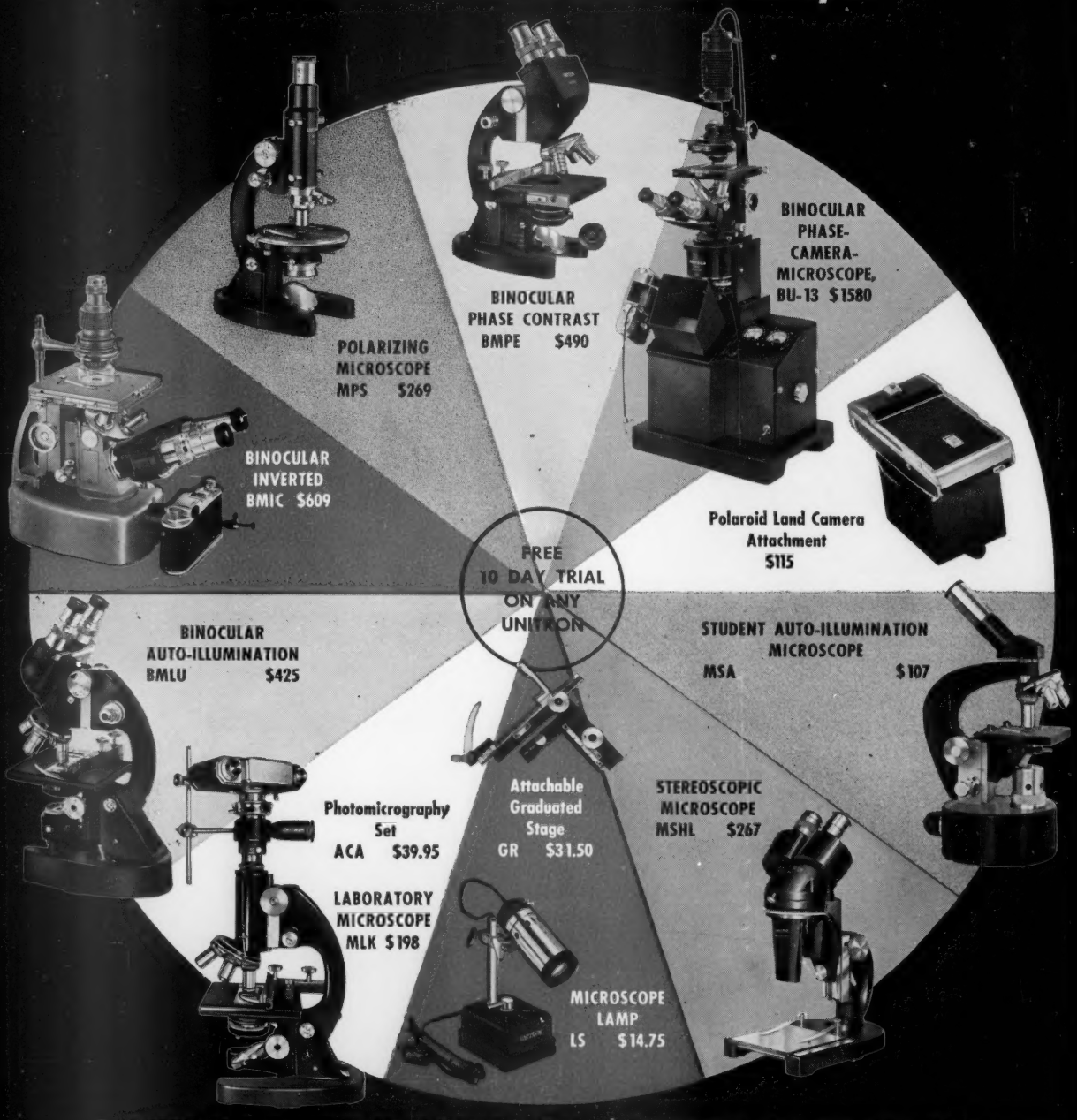
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Junipers at timberline. Photograph by Cedric Wright, from the book *This Is the American Earth* by Ansel Adams and Nancy Newhall (Sierra Club, 1050 Mills Tower Building, San Francisco, Calif.). Reviewed on page 1759.

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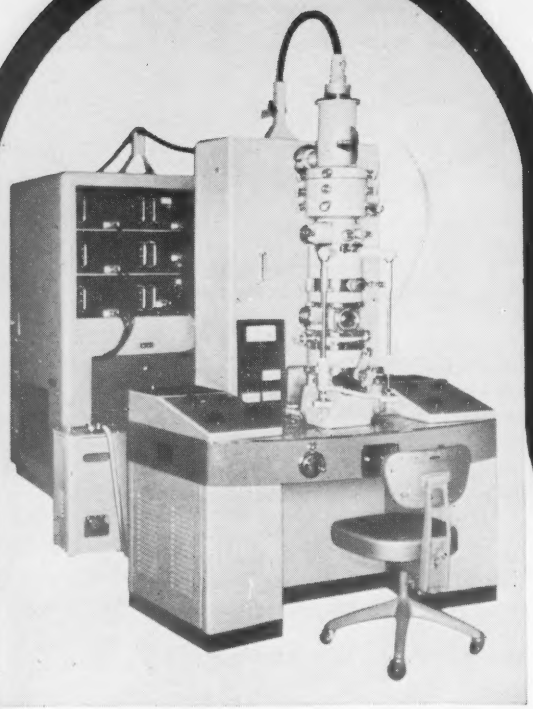

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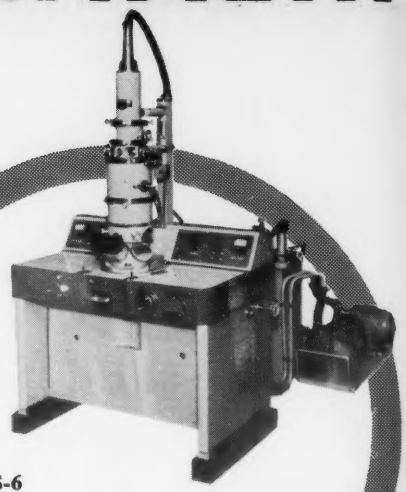

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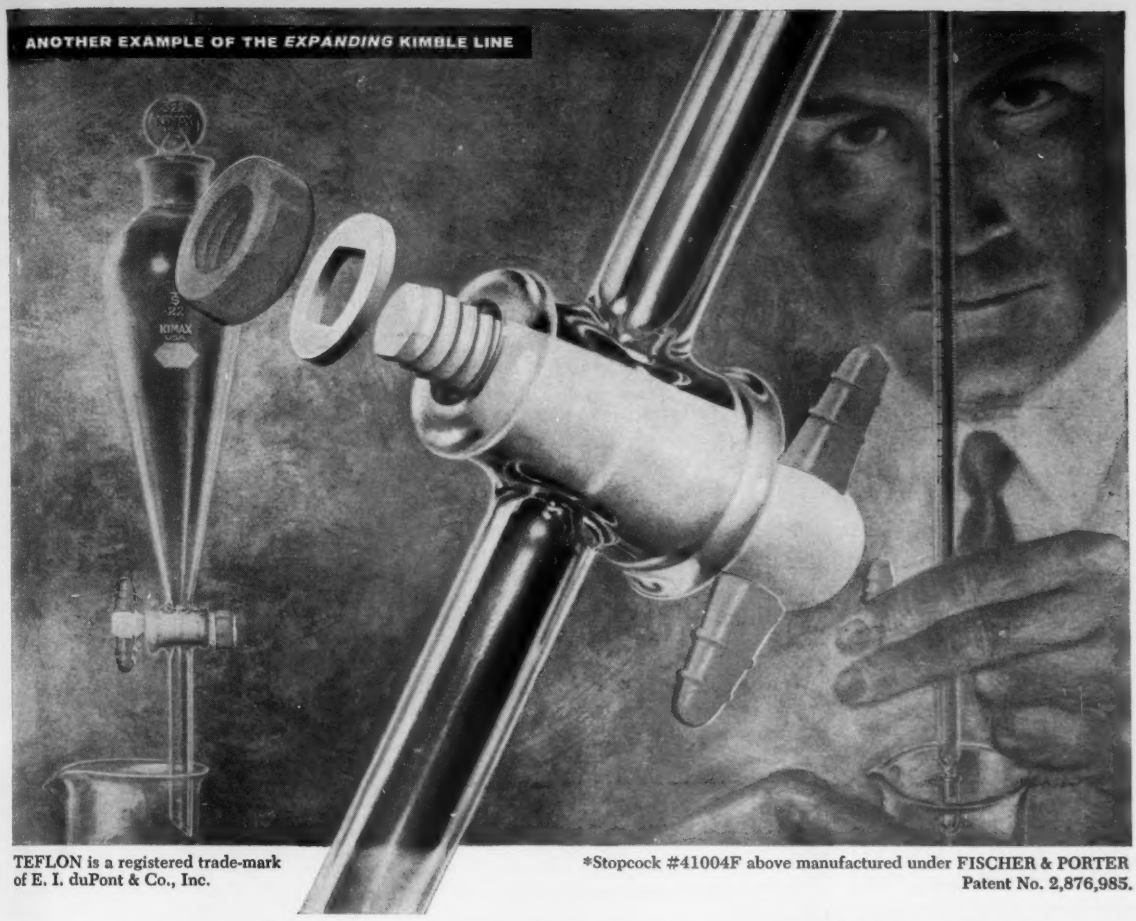

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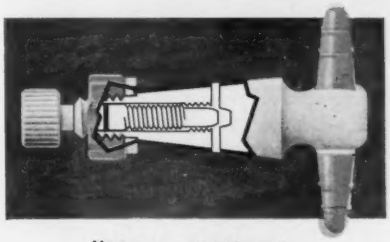

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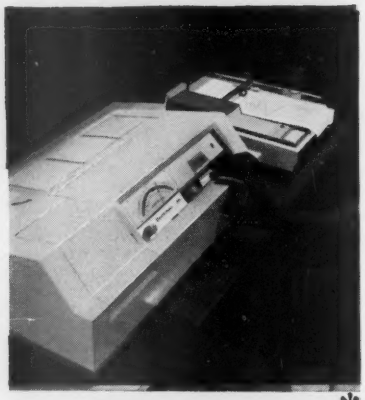

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Letters

Science and the Welfare of Mankind

I am more encouraged than I have been in years about the state of the world and its prospects for the future by the report of the AAAS Committee on Science in the Promotion of Human Welfare [*Science* **132**, 68 (8 July 1960)]. This report offers the first prospect I have seen of achieving a source of dispassionately accurate, reliable, and undistorted information on the new environmental factors of our age which are subtly but vitally affecting the course of human life throughout the world.

This country, a leader both in scientific achievement and in moral heritage, has a responsibility to humanity to understand and to state clearly to all men the facts about the reactions on human beings of the changes technology is making in our environment. Unfortunately, in many areas, this country's private industrial and merchandizing interests have seen fit to obscure or distort many of these facts. Government appears to be either unwilling or unable to make major contributions to public enlightenment.

Until I saw the report of the AAAS committee, the situation seemed pretty hopeless. Those who speak out are lone voices in the wilderness, and "authorities" are available on both sides of every issue. Nowhere can one find a source of information on the truly vital issues of our age which can be viewed as impartial, scientifically sound, and interested only in the ultimate welfare of humanity.

The special committees recommended by the Committee on Science in the Promotion of Human Welfare should provide such a source of information. I wish, therefore, to urge the AAAS to give the highest possible priority to implementation of the committee's recommendations. The accumulation and dissemination of this information is, in my view, the most important single project currently proposed by any organization in the United States. It is a project the ultimate value of which to the welfare of mankind is utterly incalculable. It should be undertaken immediately and pressed with the utmost vigor consistent with scientific accuracy.

VERNON M. ROOT
Applied Physics Laboratory,
Johns Hopkins University,
Silver Spring, Maryland

I have read with great interest the report of the AAAS Committee on Science in the Promotion of Human Welfare, and I find it singularly objective in tone and very much to the point in content. At a time when so much is being said and written on the political responsibilities of scientists from the adolescent angle of a Calamity Jane, it is most refreshing indeed to read a report on the same subject which is mature in its approach.

As the committee asks for recommendations from colleagues regarding "questions of immediate importance" for further study and discussion, may I suggest that to the six listed there be added the following: The relationship of science to the other aspects of human culture. For example, what is the relation of science to philosophy, to art, to religion? The problem of their interrelations is doubtless implicit in the whole report, but I think the problem itself deserves special attention. In any case, as I see it from a philosophical standpoint, we can hardly expect to achieve the goal of reducing the growing gap in our culture between the sciences and the humanities without a serious consideration of their similarities and differences. The attainment of such a goal will require, of course, closer cooperation between scientists and nonscientists in the years to come.

PATRICK ROMANELL
Medical Branch,
University of Texas, Galveston

The report of the Committee on Science in the Promotion of Human Welfare is the best product of the scientific method that I have seen for a long time.

Surely, since it is as a result of the progress of science that the world is in its present predicament, it can be argued that scientists have an obligation to humanity to devise methods of salvation.

Those scientists (if there be any such) who feel no such obligation may also agree with the report, as a matter of self-preservation, for it is very certain that if the problem of nuclear war is not solved there will be no science and few scientists left.

Nuclear war is only one of the problems presented to society by science, but it is the most pressing, for if it is not solved soon, none of the others will need attention.

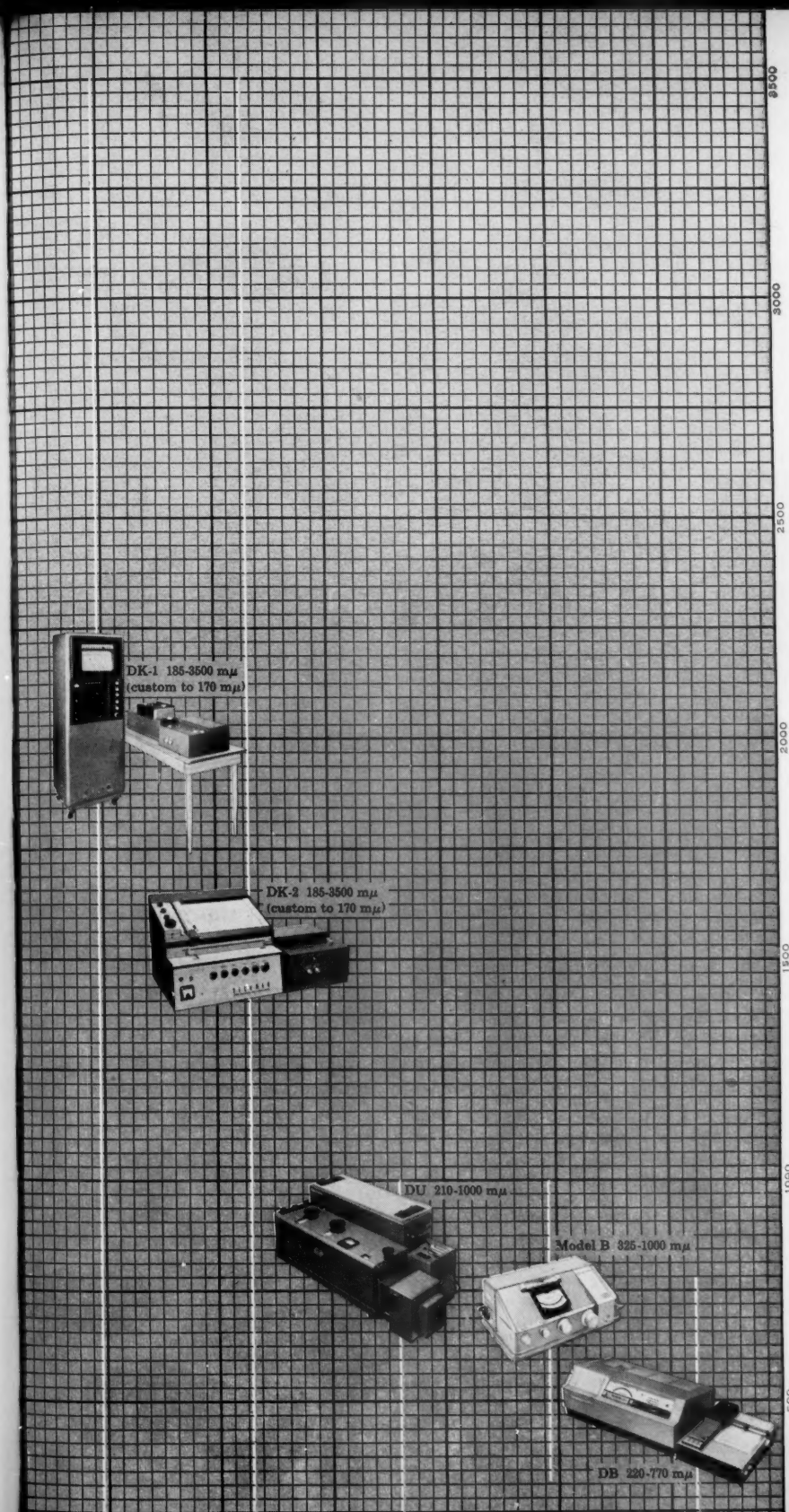
IRVING F. LAUCKS
Post Office Box 607,
Healdsburg, California

Teaching and Learning

I would like to comment on John Helwig's letter regarding the training of college teachers [*Science* **132**, 845 (23 Sept. 1960)].

I share his concern that state licensure might lead us to so much emphasis on

(Continued on page 1772)



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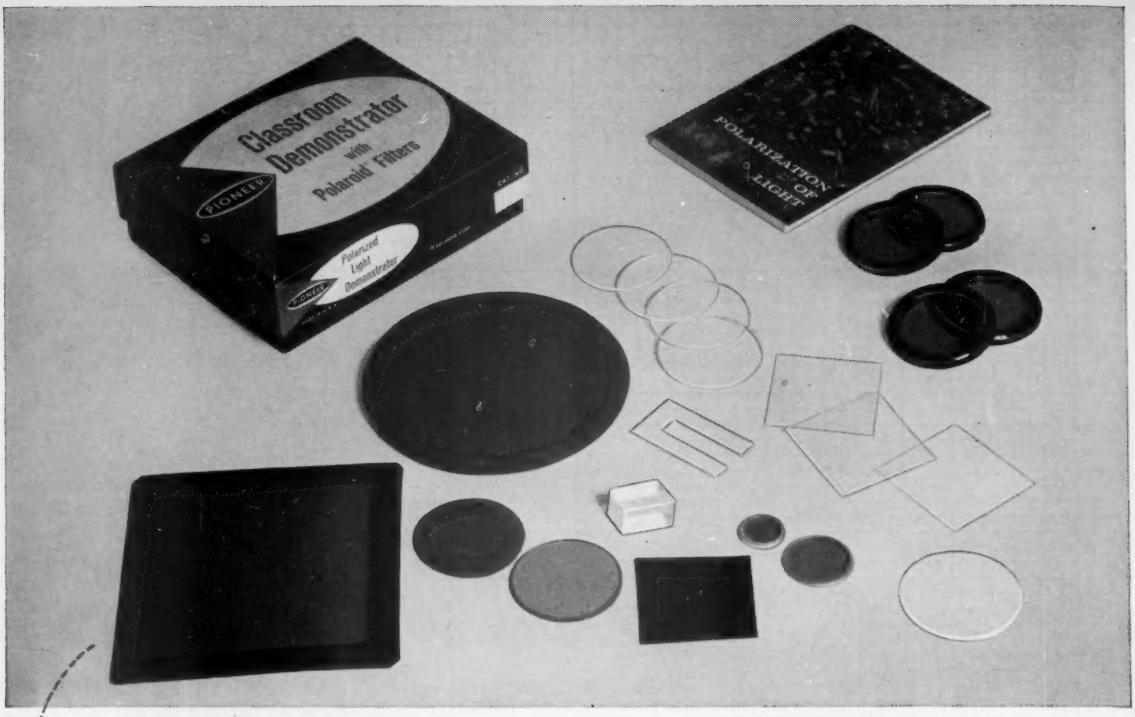

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Editorial and personnel-placement correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D.C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors. For detailed suggestions on the preparation of manuscripts and illustrations, see *Science* **125**, 16 (4 Jan. 1957).

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Assisting Less Developed Countries

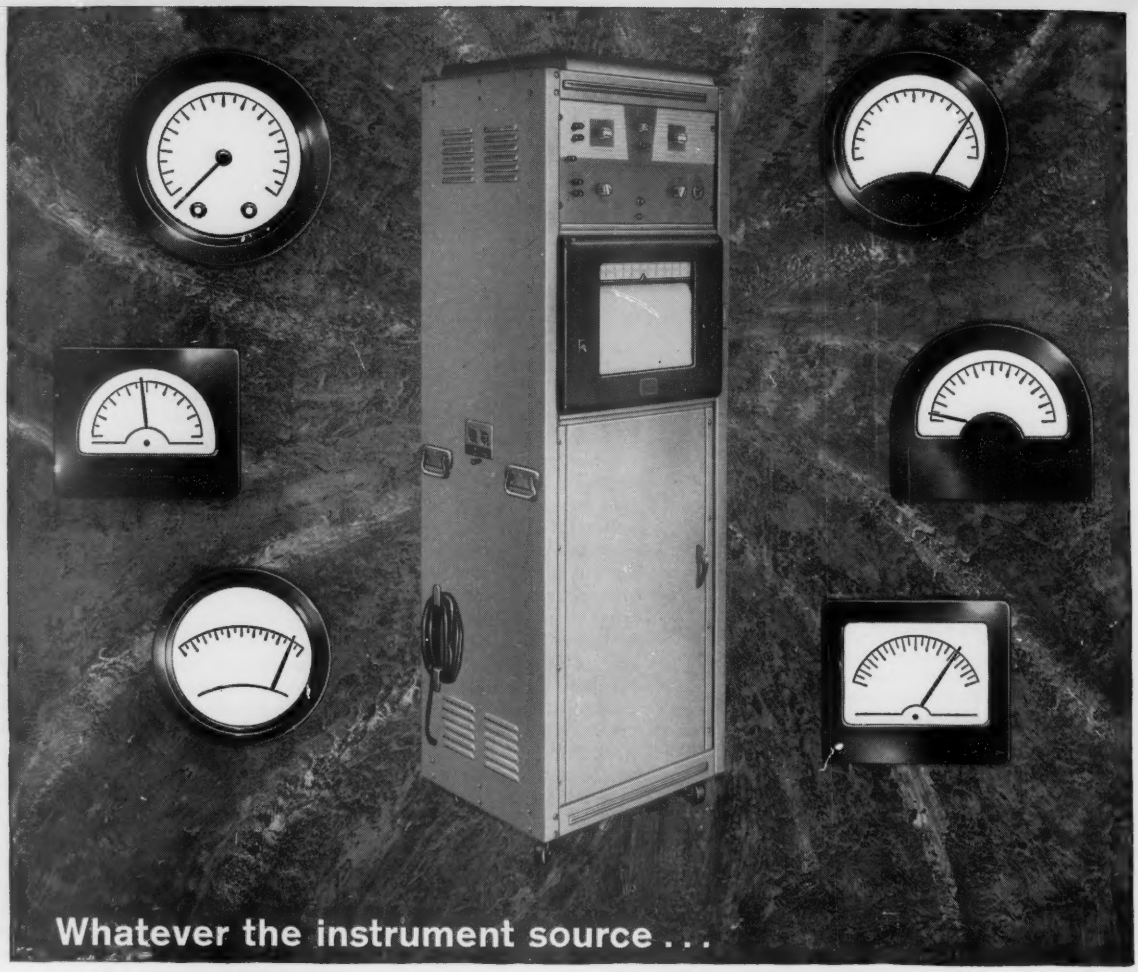
It is well known that scientific thought and technical skill have an important bearing on our efforts to help less developed countries achieve higher standards of living. The uses of research in such fields as agriculture, nutrition, and epidemiology are familiar. In plant breeding, for example, a new hybrid rice may be produced by combining the higher response to fertilizer of one variety with the adaptability to tropical conditions of another. Also familiar are some of the spectacular developments that physics is expected to produce in the next few decades, like breeder reactors, which generate more fissionable material than they consume.

But there is still much known technology that is not being used, even though relevant to the problems of backward countries, and there is plenty of room for the development of new devices of more immediate applicability than nuclear reactors. Some indication of the range of possibilities was given at the sessions last August of the International Conference on Science in the Advancement of New States, held in Rehovoth, Israel. On the question of sources of power, for example, ideas included the development of special, low-maintenance electric generators to provide light for remote villages, while in the matter of increasing the water supply, there was an account of recent work in controlling evaporation from lakes by spreading a substance on the surface that forms an extremely thin, blanketing layer.

In education, also, the Rehovoth conference brought out the possibility of adapting developments in other lands to the special needs of less developed countries. Education is central to effective technical assistance, and education now means not just literacy but scientific literacy. Mention was made of the work of the Physical Sciences Study Committee in the United States in creating a new high school course in physics. What was distinctive about this project was not just the incorporation of a modern viewpoint, but the massive effort that was mounted to carry out the work. There was not only the cooperation of university professors, high school teachers, writers, and others in the preparation of text and apparatus, but also the special training at summer institutes of the teachers who were to give the courses. Science courses designed for less developed countries might prove different in scope, but the same approach might be used in their preparation.

Also discussed at the conference was the possibility of doing more research in the social sciences. Success in technical assistance has something to do with our knowledge of the attitudes and beliefs of the people we hope to assist. In fact, as the conference made clear, to speak only of introducing new techniques is to understate the problem. Technical assistance involves at bottom introducing a new world outlook, and the problem is that rapid cultural change can be an extremely disruptive social force.

The feeling is growing in this country that we should increase the assistance that we are now offering less developed countries. But there must also be growth in appreciation of the role that science can play in making the assistance effective. Just as surely as there are many research projects now underway that could profitably be expanded, so there must be many ideas, as yet unborn, that greater support and interest would quickly produce. What is needed is a rather special technology, one suited to the requirements of growing states but perhaps of no more direct use to American citizens, if nonetheless necessary, than a Polaris-launching submarine.—J.T.



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CURRENT PROBLEMS IN RESEARCH

The East Pacific Rise

Recent marine geophysical measurements have shed some light on processes acting in the earth's mantle.

H. W. Menard

The sea floor slopes downward from California to Hawaii, and there is no obvious reason why it should. Ten years ago it seemed not unreasonable that the floor of an ocean basin should slope downward from the continental margin to the center. True, the deepest parts of the oceans are at the margins, but in many places, notably the east coast of North America, a great apron of continental debris forms a transitional slope between continent and ocean. As cores of sediment and seismic stations measuring the thickness of sediment accumulated, it became apparent that, off southern California, the apron of continental debris is either very narrow or nonexistent, and the sediment of the deep-sea floor is of relatively uniform thickness. The underlying layers in the earth's crust are also of relatively uniform thickness, and consequently the top of the mantle is parallel to the sea bottom. The slope between California and Hawaii is apparently produced by some variation in the mantle itself.

The fact that this was not an isolated problem was established immediately before and during the International Geophysical Year, when the Scripps Institution of Oceanography undertook to explore the East Pacific Rise. This rise, a vast bulge in the sea floor, extends through the remotest waters on earth and was little known; even the general outlines of the topography had only just been established by U.S. Navy ships during Operations Highjump and Deepfreeze. Scripps geological expeditions had already reached the fringes of the rise in regions east of the Marquesas Islands and west of Central America and found abnormal heat flow in both places and abnormal seismic velocities in the mantle at one of them. Comparable anomalies had been found in the North Atlantic, where the Mid-Atlantic Ridge is conveniently located for exploration. Maurice Ewing (*1*) and his collaborators at Lamont Geological Observatory found abnormal seismic velocities, and Sir Edward Bullard of Cambridge University found very high heat flow at the crest of the ridge. Were these few values characteristic of oceanic rises and ridges in general?

Another question to be investigated was raised by a hypothesis just then proposed by Ewing and Bruce Heezen. It had been known for many years that the Mid-Atlantic Ridge, the Mid-Indian Ridge, and the various ridges and rises of the Pacific basin form an almost continuous topographical feature marked by shallow earthquakes. Heezen and Ewing discovered a central rift valley associated with the seismic belt at the crest of the Mid-Atlantic Ridge in the North Atlantic, and they proposed (*2*) that the earthquakes originated in the rift valley and, since the topographic highs and the seismic zone extended through the oceans, that the rift must be continuous for 80,000 kilometers. If so, it would be present in the area to be studied in the parts of the East Pacific Rise which were about to be explored.

The Scripps Expedition Downwind was designed as a field experiment to investigate the known questions about topography, crustal structure, and heat flow of the East Pacific Rise as well as a number of problems of physical oceanography, geochemistry, and biology. The results (*3*) of this expedition and of others before and after (Fig. 1) can be combined to give a progress report on research on the East Pacific Rise. Attention is focused here on three questions. Does the rise extend under western North America? Are transverse wrench (tear) faults part of the rise structure? What is the origin of the rise?

Location and Topography

The East Pacific Rise is a vast low bulge of the sea floor comparable in size to North and South America (Fig. 2). The average relief is 2 to 3 kilometers, exclusive of the additional local relief of features such as volcanoes. The rise is 2000 to 4000 kilometers wide, and from Mexico to New Zealand it is 13,000 kilometers long.

Transverse to the East Pacific Rise are a number of long straight bands of mountainous topography called fracture zones (*4*). The crust of the rise is offset or changes trend in several places where it is intersected by fracture zones. In addition, the whole width of the rise has been displaced vertically by several hundred meters along some fracture zones. Thus, it appears that the vast bulge of the rise has been sliced

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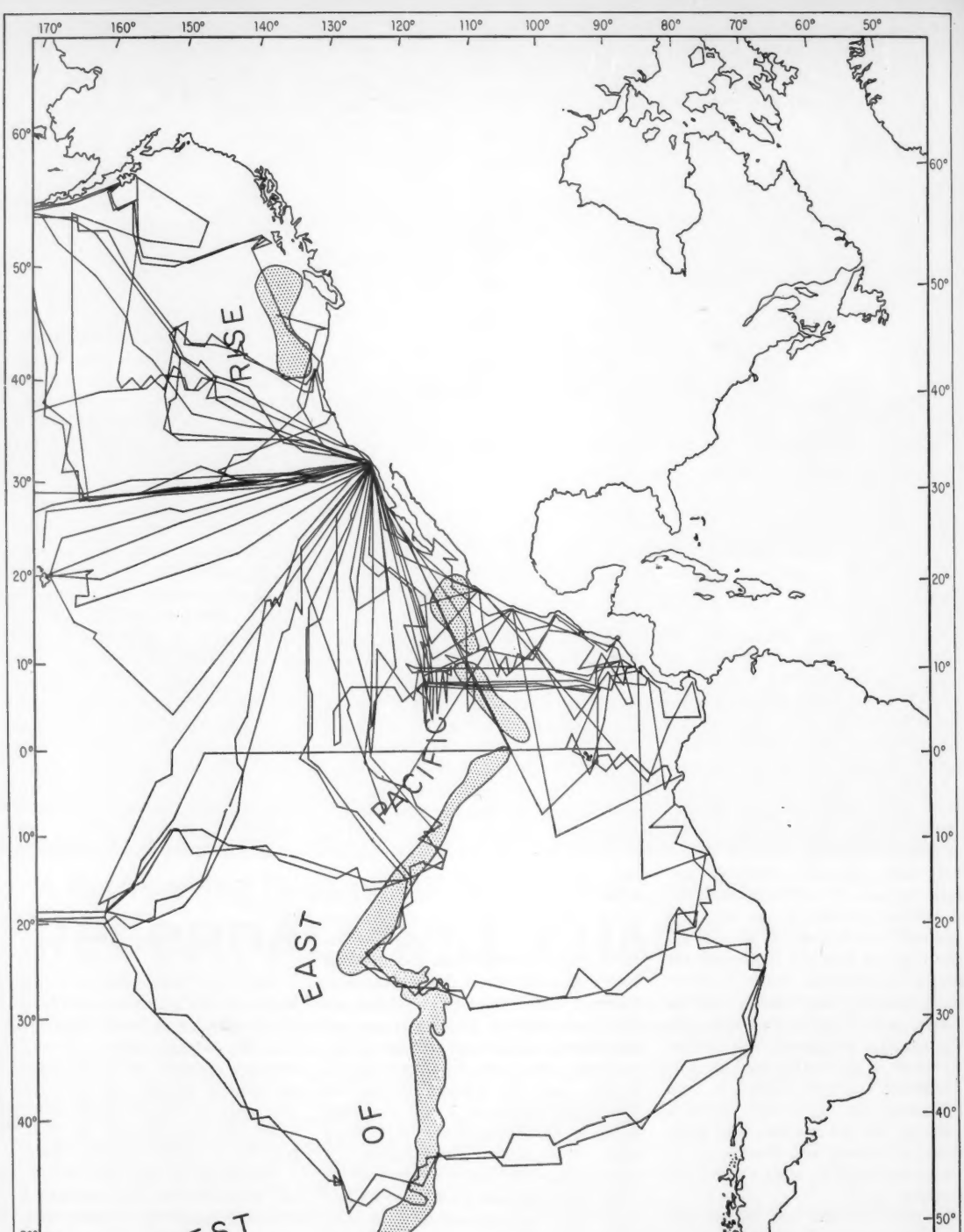

Fig. 1. Scripps Institution of Oceanography sounding and sampling lines in the eastern Pacific.

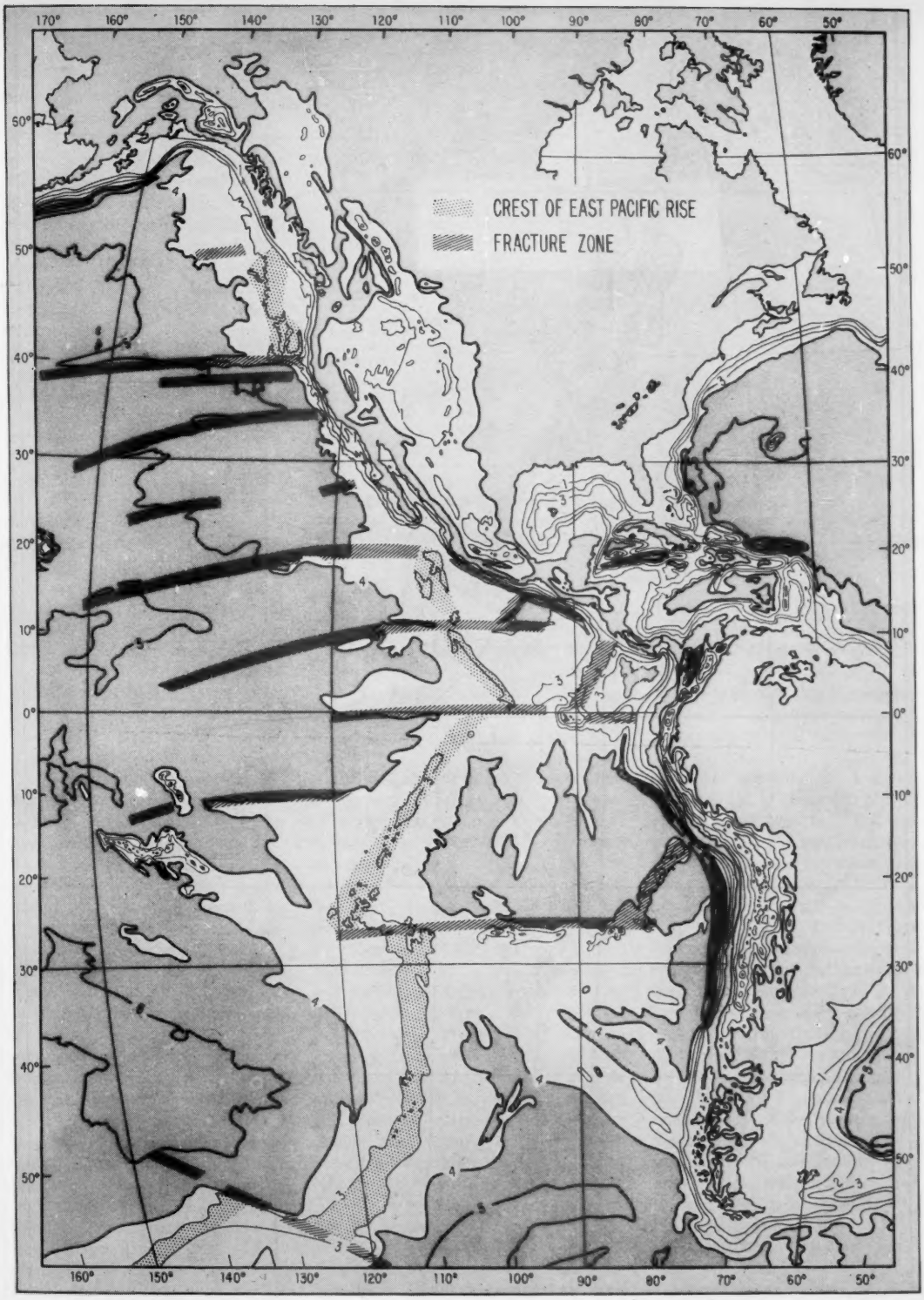

Fig. 2. Topography of the eastern Pacific and the Americas (depths and elevations in kilometers). Areas shallower than 4 kilometers in the ocean or higher than 1 kilometer on land are shown in white and are considered anomalous. On this basis the crest of the East Pacific Rise appears to extend for some distance through western North America, and the western slope of the rise is continuous as far north as Alaska.

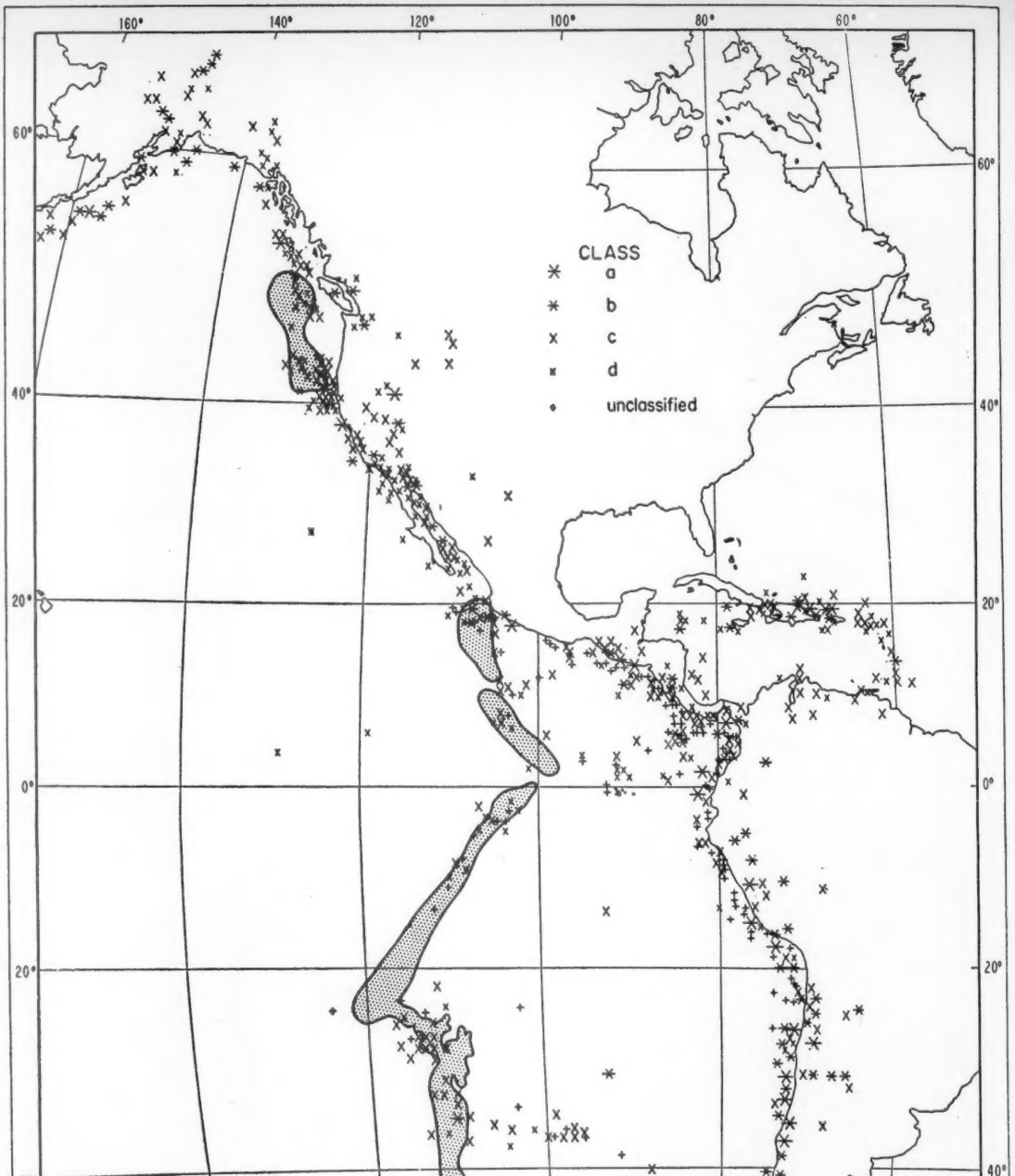

Fig. 3. Distribution of shallow earthquakes along the East Pacific Rise.

into sections which have been moved.

The fracture zones of the eastern Pacific characteristically have a relief of a few kilometers, a width of a few hundred kilometers, and a length of several thousand kilometers. Basically, they can be visualized as broad welts separating regions with different depths. Very long straight faults parallel to the welt have cut the center of each welt. The straightness of the faults suggests that the dominant displacement has been wrenching or horizontal movement. However, the only displacements evident from topography in most places are vertical ones, which have more of a tendency to produce relief. The central parts of the welts have been broken up in one of two ways. Typically, on the eastern ends of the fracture zones, only half of a welt is evident, and it is bounded by a cliff as much as 3 kilometers high. In contrast, the western parts of the fracture zones generally have a welt with the center collapsed in a band of elongate fault blocks forming ridges and troughs (graben). Large submarine volcanoes and islands are also common in fracture zones. For example, the Clarion fracture zones grade from west to east, from a welt with a central graben, to a graben interrupted in places by volcanoes, to a group of volcanic islands. On shore, along the same trend, are the line of great volcanoes of central Mexico. Fracture zones also separate topographic provinces in which volcanoes are common from provinces in which they are rare.

The oceanographers of Lamont Geological Observatory have now traced the median rift system discovered by Ewing and Heezen in the North Atlantic through the South Atlantic, where more than one rift exists, and into the Indian Ocean. English and German oceanographers have found gaps in the rift in some places in the North Atlantic, and in other places detailed surveys have shown it to be discontinuous. Results of the search for a rift in the East Pacific Rise during Downwind, Dolphin, and Doldrums, the three Scripps IGY expeditions, have been negative, despite repeated crossings of the crest of the rise. Indeed, in the Southern Hemisphere the crest of the East Pacific Rise is exceptionally smooth compared with the normal hilly or mountainous local relief of the sea floor. In other places the crests of median elevations of the ocean basins typically have been faulted into graben and horsts trending roughly parallel to the rises. This type of topography is prominent in the Ridge and Trough province off Oregon and Washington.

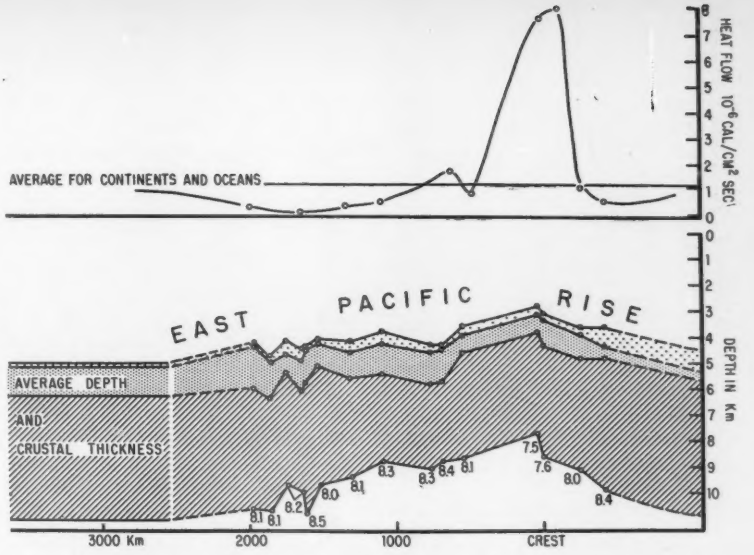

Fig. 4. Crustal section and heat flow profiles of the East Pacific Rise. Velocities below the "crust" are in kilometers per second. The crust is thinned under most of the rise, but the region of very high heat flow and anomalous "mantle" velocities is confined to a narrow band on the crest.

Seismicity

Shallow earthquakes are common along the crest of the East Pacific Rise (Fig. 3). Uncertainties in fixing epicenters in the southeastern Pacific make it impossible to establish whether the earthquakes are precisely aligned along the crest. Ewing and Heezen have found a very close correspondence between epicenters and the crest of the Mid-Atlantic and Mid-Indian ridges, and they have suggested a similar relationship in the Pacific. I have pointed out that earthquakes are more common within fracture zones than elsewhere in the eastern Pacific basin, but it now appears that this may be an incidental effect of the intersection of the East Pacific Rise seismic belt with transverse fault structures.

Crustal Section

An extensive network of shipborne seismic stations has been occupied by the Scripps geophysicists R. W. Raitt (5) and G. G. Shor in the eastern part of the Pacific Ocean. Results obtained at these stations show that the topographic bulge of the East Pacific Rise is matched by a very similar bulge of the mantle. However, it appears from present data that the mantle bulge has somewhat greater relief (Fig. 4). The oceanic crustal section consists almost everywhere of a layer of unconsolidated sediment a few hundred meters thick, a "second layer" of consolidated sediment and volcanic rock which is variable but, roughly, 1 kilometer thick, and a "third layer" or oceanic crust proper, which is separated from the mantle by the Mohorovičić discontinuity. The average thickness of the third layer at the 21 seismic stations within the Pacific basin, as defined by the andesite line, but off the East Pacific Rise is 4.9 kilometers. On the other hand, the average thickness of the third layer at seven stations within 1400 kilometers of the crest of the rise is only 3.7 kilometers. An additional group of five stations very near the crest of the rise does not obtain normal mantle velocities (8 km/sec or more) below the third layer, and there is a question about calling the third layer the "crust." However, the third layer, as such, at all 12 stations within 1400 kilometers of the crest, has an average thickness of only 3.8 kilometers, or 1 kilometer less than that found at an average station elsewhere in the Pacific basin.

Heat Flow

The intensity of heat flow through the crust of the East Pacific Rise has a close correlation with the topography, according to numerous observations by Richard von Herzen (*6*), Arthur Maxwell, and Roger Revelle. Average heat flow on continents and in ocean basins away from rises is about 1.1×10^{-6} cal/cm² sec. Along the crest of the rise the heat flow ranges between 2 and 8×10^{-6} cal/cm² sec in a band a few hundred kilometers wide and 10,000 kilometers long (Fig. 5). Similarly, high heat flow occurs on the crest of the Mid-Atlantic Ridge. In marked contrast, an area as much as 3000 kilometers wide and 6000 kilometers long on the west flank of the rise has low heat flow, with values of 0.14 to 0.97×10^{-6} cal/cm² sec. Another belt of low heat flow appears to exist on the east flank of the rise, but there have not been enough observations for us to be sure. The Downwind expedition was designed to investigate a suspected correlation of high heat flow with the crest of the East Pacific Rise, and this has been confirmed. The belt, or belts, of low heat flow had not been suspected, and another expedition will be required to work out the pattern in the eastern Pacific.

Does the Rise Extend under Western North America?

Now that the general characteristics of the East Pacific Rise have been described, the question of its continuation under western North America may be considered.

With regard to topography, it should be emphasized that this rise is only the Pacific section of a system of mid-ocean elevations which extends around Antarctica into the Indian and Atlantic oceans. An abrupt end to the system off Mexico would thus be all the more remarkable. However, the maps show that, although the crest of the rise can be traced as a distinct submarine feature only as far north as the latitude of Mexico, the western flank continues up to Alaska, and the crestal type of topography reappears off Oregon and Washington as the Ridge and Trough province. The puzzling slope between California and Hawaii is the west flank of the rise, and much of the geological exploration of the eastern Pacific has been on different parts of the same great feature. Where the crest and east flank of the rise intersect Mexico are found the plateau of Mexico, the Colorado Plateau, and the Basin Ranges, comprising a topographic bulge of the continent comparable in scale to the bulge of the sea floor. Similar plateau highlands exist only in central Asia and in east Africa, and the latter occurrence lies along a continuation of the rise in the Indian Ocean.

As for seismicity, the continuity of a belt of shallow earthquakes from the the crest of the East Pacific Rise south of Mexico, through the Gulf of California and California, to the Ridge and Trough province cannot be questioned. In California, however, many of the earthquakes are along the San Andreas fault, which may be unrelated to the stresses producing the rise faulting.

The typical thin oceanic crust of the rise reappears in the Ridge and Trough province, according to preliminary shipboard calculations by Raitt. This type of crust cannot, of course, be identified in the intervening continent.

The high heat flow characteristic of the East Pacific Rise south of Mexico has also been found by von Herzen in the Gulf of California, in the continental borderland off southern California, and in the Ridge and Trough province. The discontinuity under California appears to be a relatively trivial break in this long belt of high heat flow associated with the crest of the rise.

In summary, the East Pacific Rise, identifiable as an elevated region with a seismically active crest and high heat flow, extends from the south Pacific, under western North America, into the northeasternmost Pacific. The corresponding elevation of the mantle that is found in the oceanic regions has not been found under the intervening continent.

Possible Genetic Relationship between Rise and Fracture Zones

The great structures of the eastern Pacific basin are the East Pacific Rise and the system of intersecting fracture zones. Various lines of evidence suggest that the structures are genetically related—namely, similar geographical position, similar scale of crustal displacement, and interaction.

The fracture zones of the eastern Pacific form a pattern of rather regularly spaced and approximately parallel lines distributed in an area of about 10 percent of the earth's surface. As more sounding lines are collected it appears increasingly definite that most of the fracture zones do not extend as prominent features beyond the flanks of the East Pacific Rise, thus suggesting a genetic relationship. However, the possibility remains that these very extensive fractures are part of an ancient global system which has been rejuvenated and accentuated in the eastern Pacific. Lineations resembling fracture zones have been described in many places in the world, and the whole pattern in China is strikingly similar. Various worldwide fracture systems have been described, notably by F. A. Vening Meinesz (*7*), but the east Pacific zones, as a group, do not fit into any such pattern as yet proposed.

A comparison of crustal displacements associated with fracture zones and deduced from the structure of the rise requires a detailed description of available observations. Topography alone merely suggests that fracture zones are wrench faults, but a series of remarkable maps of the earth's magnetic field in the northeastern Pacific, prepared by Scripps geophysicists, not only proves the direction of faulting but gives the amount along several fracture zones. The first maps made by Ronald Mason (*8*) show a very extensive pattern of distinctive magnetic anomalies trending very close to north-south. The pattern is offset 150 kilometers in a right-lateral direction at the Murray fracture zone (which may now properly be called a fault). Stimulated by this discovery, Victor Vacquier began a series of magnetic surveys to establish whether displacements could be measured on other fracture zones. To date, he has found a 250-kilometer, left-lateral displacement of the Pioneer fault and a 1200-kilometer left-lateral displacement on the Mendocino fault (Fig. 6). This last displacement is by far the largest that has been measured on earth. Indeed, it is difficult to see how such large displacements could be established except in the very favorable circumstances which exist in the eastern Pacific basin. A similar displacement would tear a continent in half.

Depth contours in the area of the magnetic maps are offset in the same directions as the magnetic anomalies. To a remarkable degree, the amount of offset is also similar. The coincidence in offset is best demonstrated by the 4500-meter contour, which is least affected by the deposition of sediment near the

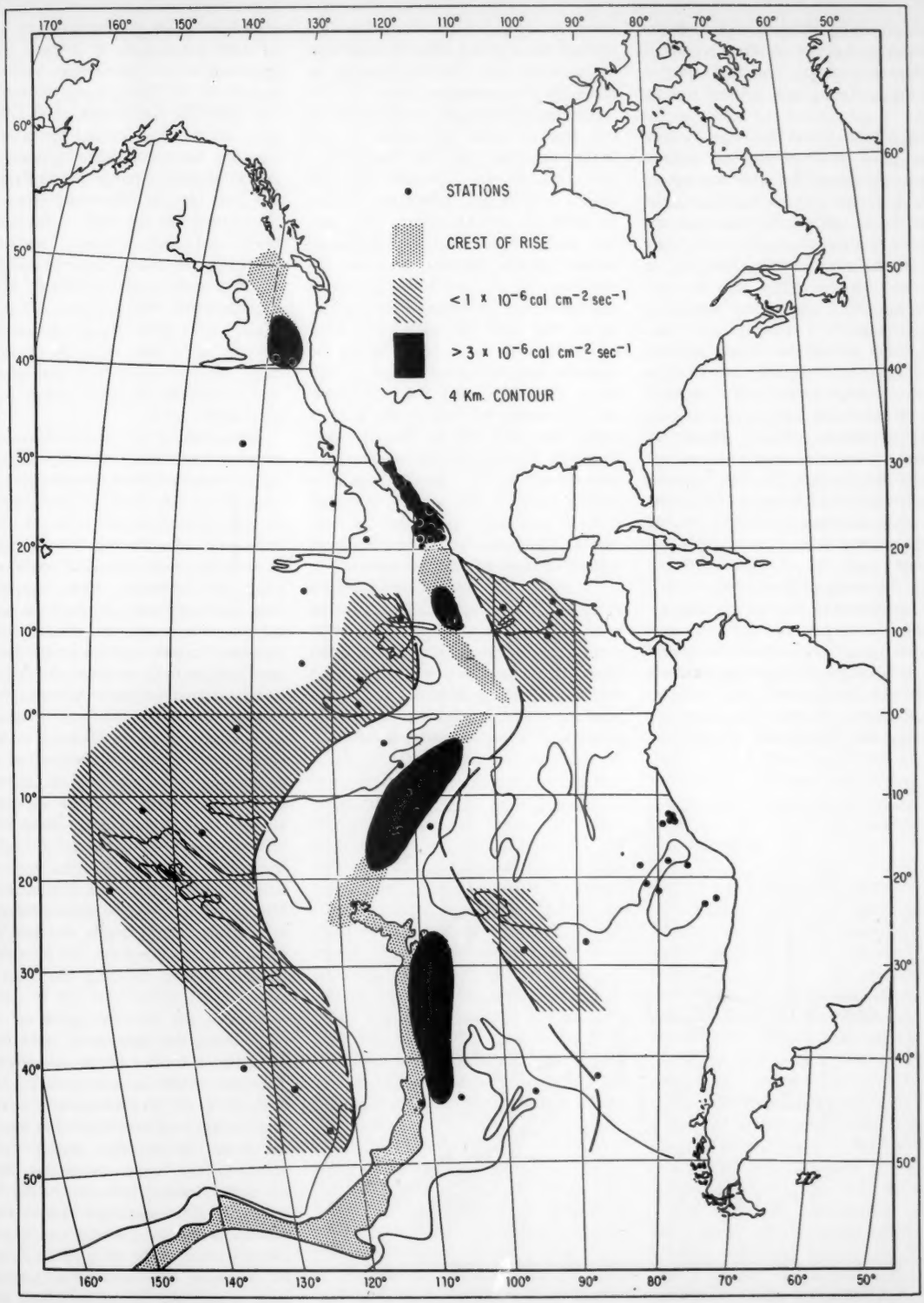

Fig. 5. Correspondence in position of the crest of the East Pacific Rise and a pattern of heat flow. The points off the United States are based on unpublished observations by von Herzen. The band of high heat flow, like the crest of the rise, appears to pass through the western United States.

continent. It is difficult to escape the conclusions that the sea floor was sloping before the offsets occurred and that the displacements were almost exactly level.

The crustal blocks between fracture zones have areas of a few million square kilometers, but they are hardly more distorted by displacement than small blocks of wood. The magnetic anomaly pattern shows little evidence of compression, extension, rotation, or shearing, except within the fracture zones. However, a distinctive pattern of minor lineations (*9*) suggests a uniform stress within each block but different stresses in adjacent blocks. The lineations consist of elongate volcanoes, lines of volcanoes, and mountains produced by block faulting. These are features characteristically associated with crustal tension. It would appear that all the crustal blocks were in tension while the eastern Pacific was being sliced, or after it had been sliced, by fracture zones.

The enormous displacements of marine crustal blocks are not matched on the adjacent continent. The Mendocino fracture zone offsets the continental slope 100 kilometers in northern California, but the displacement is right-lateral—that is, in the opposite direction from the displacement of the deep-sea floor farther to the west. The continental slope is not offset by the Murray fracture zone, but the Channel Islands and the transverse ranges of California appear to mark an extension of the fracture zone on land. Wrench faults with the trend of the Murray fracture zone are common in these islands and ranges. Displacements are as much as 15 kilometers, but they are generally left-lateral—once again in the opposite direction from the displacement of the sea floor. It appears that some zone of discontinuity is parallel to, and near, the continental slope off California; that crustal blocks on opposite sides of the zone move in different directions; and that movement on the continental side of the zone is much less than on the oceanic side. However, the ages of the varying displacements are not known, and it is equally possible that wholly unrelated stresses produced movement in different directions at different times along the same zone of weakness.

To turn to wrench faulting deduced from the structure of the rise, if the boundary between crust and mantle marks chemically different rocks, the thin crust of the rise probably has been stretched. Arching of the crust would produce some thinning, but the amount is trivial. The region which is now 3.8 kilometers thick has an average width of 2800 kilometers. If it once had a thickness of 4.9 kilometers, which is typical of the Pacific basin outside the rise, then the width was 2200 kilometers, and the crust on the flanks of the rise has been translated laterally for 600 kilometers. Changes in direction of the crest require differential movement of sections of the rise, with wrench faults between sections. Moreover, wrench (tear) faults are commonly associated with large horizontal crustal displacements, both on land and in the ocean basins. Thus, the development on the rise of a thin crust seems to require wrench faults with the location and magnitude of displacement found in fracture zones.

Interaction of rise and fracture zones in space and time is strongly suggested by the seismicity and topography. The crest of the rise is seismically active at present, indicating that it is still being deformed. The fracture zones appear to be active only where the crestal seismic zone intersects them, suggesting that their apparent seismicity is coincidental. However, the fracture zones are very straight, and the crustal blocks are undistorted, despite their great translation; the wrench faults may be almost completely frictionless discontinuities in the crust. Perhaps faulting occurs after only very small strain accumulates, with resulting earthquakes below the normal level of detection. George Shor is planning to build small seismometers which can be placed on the sea floor in the fracture zones to detect small earthquakes if they exist. Meanwhile it would be quite intriguing to employ a bathyscaphe and look for sinuous bends along the old submarine telegraph cable crossing the fracture zones.

Even if the fracture zones are not now active, they appear to have been since the rise first began to develop. The crest of the rise is offset or changes direction at the intersection of the fractures, suggesting horizontal movement; the flanks of the rise change depth along fracture zones, suggesting vertical or horizontal movement; and between the Mendocino and Murray fracture zones the flank of the rise is horizontally offset in the same direction and by about the same amount as the magnetic anomalies. If an anticlinal fold on land were offset by wrench faults in this manner it would be obvious that the wrenching had occurred after the

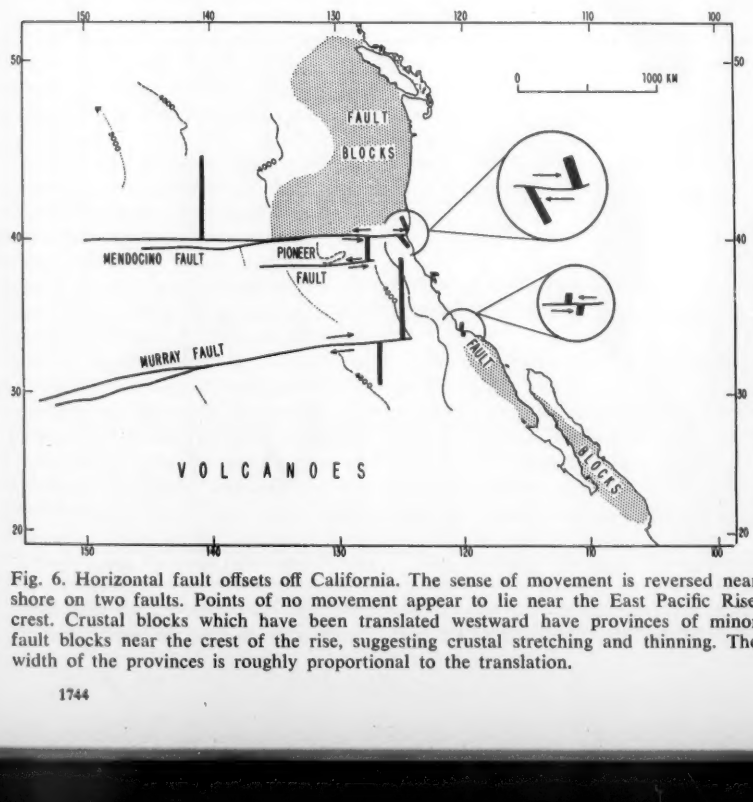

Fig. 6. Horizontal fault offsets off California. The sense of movement is reversed near shore on two faults. Points of no movement appear to lie near the East Pacific Rise crest. Crustal blocks which have been translated westward have provinces of minor fault blocks near the crest of the rise, suggesting crustal stretching and thinning. The width of the provinces is roughly proportional to the translation.

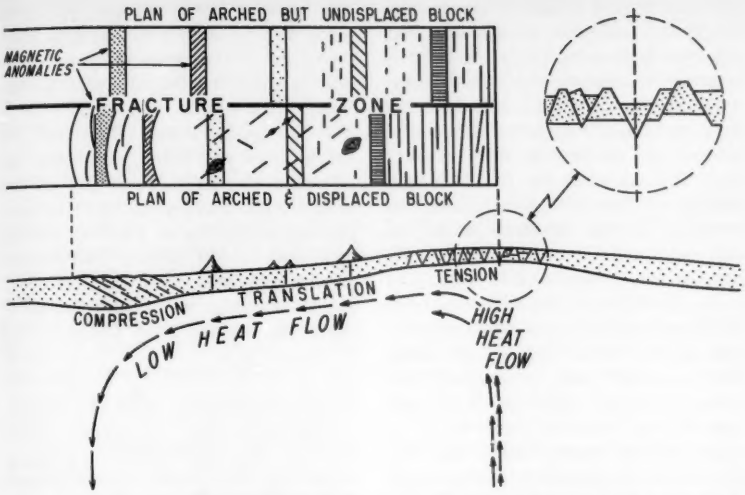

Fig. 7. A diagrammatic presentation of the convection-current hypothesis for the origin of various features associated with the East Pacific Rise.

folding or else that they were contemporaneous. It is difficult to escape the same conclusion regarding the enormous, undated, and little known structures of the sea floor.

Although no single line of evidence is wholly conclusive, the rise and fractures probably are genetically related. The thinning of the rise crust seems to require large horizontal movements on the flanks with differential movement between blocks. The fracture zones have the required geographical distribution and magnitude of displacement to be the boundaries between the displaced blocks; they have been active since the rise began to form, and the rise, at least, is seismically active and may still be developing.

Origin of the East Pacific Rise

Inasmuch as the facts available about oceanic rises are few and new information is being accumulated very rapidly, hypotheses about the origin of the rises are modified frequently or abandoned for new ones. Consequently, an elaborate discussion of present working hypotheses does not appear warranted, but a brief outline may serve the useful purpose of relating otherwise disconnected facts. The following facts are known about the East Pacific Rise.

A belt about 800 kilometers wide along the crest of the rise is anomalous in almost every respect: (i) The topography is characterized in some places by ridges and troughs trending parallel to the rise; (ii) the belt is seismically active; (iii) the heat flow is several times the normal flow; (iv) the third layer (crust) is thin; and (v) the seismic velocities are much less than mantle velocities at a depth where the mantle normally is found. This highly abnormal belt occupies about one-fifth the width of the rise.

Beyond the central anomalous belt and to about the midlines of the two flanks of the rise—at least on the better-known, western flank—the crust remains thin but the heat flow is abnormally low and the velocities in the mantle are normal. Little is known about the local topography in this region, but it appears to consist of volcanoes, low domes, and troughs with adjacent tilted fault blocks trending at various angles to the crest of the rise.

On the outer half of the western flank, the crustal thickness and mantle velocities are normal for the ocean basins, and the only abnormalities are low heat flow and the existence of the rise itself.

Fracture zones cut across the whole width of the rise and displace or change the trend of the rise. Both vertical and horizontal offsets occur, and the horizontal offsets are reversed at the continental margin.

The position of the rise along part of its length is at the edge rather than in the middle of the ocean basin, and the crest appears to pass under western North America.

A hypothesis of a youthful convection current in the mantle, suggested by Bullard, Maxwell, and Revelle (*10*) to explain high oceanic heat flow, offers a simple qualitative explanation of all the facts given above. A rising hot material, marked by high heat flow, produces an upward bulge of the mantle because of thermal expansion or physical-chemical changes (Fig. 7). The mantle bulge arches the overlying crust and forms a system of tension cracks parallel to the rise. Arching stretches and thins the crust, but the observed thinning is so great that translation of the crust toward the flanks of the rise is also required. Accordingly, the horizontal limb of the convection cell moves the crust outward and thins it at the crest of the rise by normal faulting along the tension cracks. Blocks are displaced different distances by wrench faulting on fracture zones because of variations in intensity of convection along the rise. Displacements on fracture zones have opposite directions on the two sides of the crest because convecting material moves in all directions from a rising hot center. Farther out on the flanks of the rise the convection current moves the crust between fracture zones as a unit (the displaced magnetic anomalies are not distorted as they are near the crest). On the outermost flanks the sinking convection current, marked by low heat flow, defines the outer limit of wrench faulting and, presumably, the crust is thickened by thrust faulting.

The East Pacific Rise is only one part of a world-girdling oceanic mountain system, and one may wonder whether the obvious implication of a single origin is correct. However, despite their continuity, the oceanic rises and ridges are not all alike. Compared with the Mid-Atlantic Ridge, the East Pacific Rise is much broader, has gentler slopes, and in most places is far less faulted on the crest. It may be that these differences only mark different stages of development and different times of origin. There is some evidence that the life of an oceanic rise may be rather short, and rises may be in different stages of development at the same time. All the ocean basins have median elevations except the Pacific, which, instead, has a line of atolls and guyots (different types of deeply submerged former islands) rising from a series of narrow, steep-sided ridges. If a broad oceanic rise, faulted at the crest

and capped by volcanic islands, were to subside, the only evidence that it once existed might be such a line of drowned former islands and linear ridges (*11*). The guyots of the central Pacific were islands roughly 100 million years ago, and if they mark a former rise, the period of its subsidence must have been quite short compared with the age of the earth.

The East Pacific Rise differs from most broad oceanic elevations in another respect: the northern part is at the margin of an ocean basin rather than in the center. Thus, although all ocean basins have oceanic rises or former oceanic rises in the middle, not all ocean rises are in the middle of basins. In Africa and western North America the rises appear to penetrate continents. In fact, a feature as broad and low as an oceanic rise is difficult to recognize unless there is a large, relatively flat ocean basin on each side. Several marginal oceanic regions with anomalous levels, such as Melanesia and Indonesia, may have the same mantle and crustal structure as oceanic rises.

Continental drift, as suggested by the parallelism of the Atlantic coast lines and the crest of the Mid-Atlantic Ridge, has been a very attractive concept for continental geologists, particularly since it was revitalized by the paleomagnetic evidence for polar shifts and possible drift. Marine geologists, on the other hand, have been reluctant to accept the concept of continental drift because they find no evidence for it in the geology of the sea floor. Indeed, the existence of rises centered in the Indian and Pacific oceans seemed to eliminate the possibility that Africa and South America had moved away from the Mid-Atlantic Ridge. However, if a random distribution of relatively short-lived "oceanic" rises is accepted, the picture is entirely different. If all rises were in the center of ocean basins it would not be clear whether the convection current, or another agent, which produced the rise centered itself relative to the margins of the basin or created the basin. With rises bordering the Pacific and penetrating Africa, it appears more probable that most rises are centered because the margins of the basin have been adjusted by convection currents moving out from the center. If so, the African and East Pacific rises may mark relatively young or rejuvenated currents which have not yet had time to produce much continental displacement. Even so, east Africa is being torn by deep rifts and Baja California has almost been separated from North America along the crest of the East Pacific Rise.

It is apparent that the study of oceanic rises and ridges, which was accelerated by the IGY, is one of the most fruitful fields in geology and geophysics. According to present planning there will be few, if any, months during the next three years when there will not be oceanographers at sea attempting to advance this study (*12*).

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12. Almost all the field work has been supported by the Office of Naval Research and the National Science Foundation under various contracts.

Research Overhead

DuBridge and Kaplan exchange views on overhead payments for basic research in private universities.

The article by Norman Kaplan [*Science* **132**, 400 (12 Aug. 1960)] presents such a confused and misleading argument for "stopping all overhead payments for basic research in private universities" that it will probably be ignored in most informed circles. Nevertheless, since it occupies an important place in a distinguished scientific journal, and since it represents the type of thinking that is frequently met in discussions of this subject, it seems important for the sake of the record that it be answered in some detail.

Apparently Kaplan bases his opposition to overhead payments on the following arguments.

1) "Concentration on them [overhead payments] obscures some more basic problems and postpones the search for more general solutions."

2) Since "the university has not made a profit or typically realized full costs on its storage facilities [that is, its libraries] [or] its teaching," there is no reason why it should realize full costs on research activities either.

3) Overhead payments would "clearly not solve the over-all financial crisis in the universities."

4) Full cost reimbursement would bring dangers of federal control of the universities.

There is no substance to any of these arguments.

1) The question of formulating an over-all research policy for the United States has been searchingly examined by many agencies ever since the famous Bush report, "Science—the Endless Frontier," in 1946—most recently by the President's Science Advisory Committee. Such studies are also a continuous responsibility of the National Science Foundation. There is no evidence that in any case these policy studies have been hampered or obscured by the question of full cost payment. Nearly all the serious studies have recommended that the federal government reimburse the full costs of research in universities. But far from obscuring the

main issues, such recommendations have clarified the problem of research support by emphasizing that if research is to go forward, then *all* the costs of it have to be paid by someone. Ignoring the indirect costs is what obscures and confuses discussion of research support.

2) The argument that students or library users do not pay full costs of the services they receive falls of its own weight when applied to research. The tuition charges that can be levied against students are governed by many factors outside the control of the private university—for example, ability to pay, competition from "free" public universities, the generosity of various donors of scholarship funds, and so on. There has been a growing tendency to raise such charges to the point where they do represent full costs, and indeed at some private institutions tuition payments actually represent 80 percent or more of the total income. However, just because, for strong but irrelevant reasons, student costs cannot be further increased at present is no argument against, but rather an argument for, the recovery of full costs on federal research agreements. Losing money on one operation has never been considered a sound business argument for losing money on all operations. It is vitally important that universities put more of their meager resources into the teaching function—for example, into teachers' salaries—and this they can better do if the overhead costs on research projects are reimbursed. Because students and their families commonly cannot afford to pay full costs hardly means that the federal government cannot afford such payments.

3) That overhead payment will not solve *all* financial problems goes without saying. But it does solve one—namely, finding the funds to pay the large indirect costs of research. Financial problems, like other problems, must be solved one at a time. Abandoning one solution because it is not a complete solution is a sure way to financial disaster. Universities must look to many sources for funds to continue their teaching, their research, and other activities. They look to private individuals, to corporations, to foundations, and to government. Each gift or grant made to a university is for a particular purpose or function. Gifts for scholarships cannot be used to pay the salary of the purchasing agent; neither can the income from an endowment fund given to support a professorship in history or physics. "Unrestricted funds" must be found to pay business and administrative costs and the costs of plant maintenance—and such funds are very hard to obtain. Why should not every donor be told of the *full* costs of carrying on the activity he is interested in and then invited to reimburse all costs wherever practicable? If universities had unlimited endowment funds for unrestricted use, the problem would be less critical. Apparently university administrations have been unsuccessful in convincing their faculty members that this is not the case.

4) Finally, of course, the argument that paying full costs of research brings on more vicious dangers of federal control than paying only partial costs has been shot to earth many times but keeps rearing its ugly head. The agencies that pay full costs have *not* attempted to control universities, any more than the agencies that do not. In fact, those that do pay full costs have *ipso facto* given the universities more freedom than those that do not. The reasoning is very simple: No university administration can well turn down a large grant to support the research work of a professor who has worked very hard to secure the grant. If the grant does not carry full cost reimbursement, the administration must find other funds to cover such costs. This means that other concerns of the university must suffer—for example, professors' salaries, employing a new instructor in history, maintenance of the physical plant. New unrestricted funds simply cannot always be found, or found soon enough. Hence, the university is forced to a decision to unbalance its program in some way to meet the new requirements. When full costs are reimbursed, however, no other activity of the institution need be damaged in any respect; the university is still free to pursue its program of allocating resources in accordance with its own best judgment. In fact, if the new research grant continues something that was in progress anyway (perhaps on a smaller scale), the general budget may be somewhat relieved, and the university is thus helped to attain the objectives it has itself decided to seek.

It is true that the university fiscal officers and government fiscal officers have many long and difficult arguments about how indirect costs are to be computed; it is true that university accounting procedures are often affected by government regulations. But it is also true that such difficulties arise also in cases where indirect costs are *not* fully reimbursed. And it is also a fact that such fiscal arguments do not result in substantial federal control of university *academic* policies. The problem of federal control is, in short, quite independent of the problem of full cost reimbursement. Abolishing overhead payments would neither decrease nor increase the responsibility of each university to make sure that grants from *any* source—private, corporate, or government—do not result in interference with the university's freedom to formulate and carry out its own academic policies.

Many faculty members appear to believe that the road to academic freedom is through financial poverty. However, history proves that, in fact, the opposite is the case.

I have not attempted to give a complete presentation of the case for full cost reimbursement in federal research grants. Such résumés may be found elsewhere *in extenso* (for example, in references cited by Kaplan). Nor is this an attempt to point out *all* the half truths and misstatements in the Kaplan article. It is simply an attempt to set the record straight on a few issues raised by that article.

It is true that the research function is a relatively new one in American universities—at least it is new in its present scale and importance. It is true that new issues have been raised, new policy questions must be answered. It is also true that the long-range scientific strength of America, of the free world, and of all civilization is critically dependent on the vigor and quality of the research activities carried on in American universities. It is vital to the interests of mankind that such research be continued and expanded. This can only be done if the fiscal problems encountered are faced with realism and understanding by administrations and faculties of all universities and also by all agencies of the government. To declaim against reimbursement of indirect costs because the money appears to go to the business office rather than to the professor is only to exacerbate and perpetuate the difficulties we face in strengthening our universities and their research capacity.

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Kaplan Replies to DuBridge

My article, "Research Overhead and the Universities," was more concerned with raising questions and issues than with providing any immediate solutions or practical suggestions for policy. The rather drastic suggestion that all overhead for basic research be stopped was made to draw attention to more serious and more general problems, which will be explored in much greater detail in forthcoming papers. The case against overhead payments was examined in a somewhat larger context than the usual one of financial needs. DuBridge's reply avoids the major issues with which I was concerned. Consequently, I appreciate this opportunity to clarify what I still think to be the main issues and expand on them here as space permits. At the same time, I will comment briefly on the arguments raised by DuBridge.

It is perhaps necessary to point out that I am not one of those who believes, as DuBridge suggests, that poverty helps to preserve academic freedom, either in educational institutions or for the individual faculty members. I do not believe that reimbursement of indirect costs should directly benefit the professor rather than the business office. I do not believe that universities have unlimited endowment funds for unrestricted use. I do not believe that there are many university faculty members who believe any of these things attributed to them by DuBridge. Such *ad hominem* assertions lend little weight to DuBridge's main argument. It should be possible to raise questions and to examine the case for an unpopular position without having one's motives impugned and one's intelligence questioned.

DuBridge focuses on the overhead problem as one important problem calling for a solution in the immediate future. He does so without calling into question any of the other present policies or aspects of the over-all financial support structure for research. To bear in mind these other considerations may be somewhat difficult for a college president faced with glaring deficits in his annual budget, where additional reimbursement for indirect research costs will undoubtedly alleviate the immediate problem considerably. But it is precisely for this reason that I sought to link overhead considerations to the larger problems involved.

Despite the many excellent studies of research policy with which we are all familiar, our approach to such policy has nevertheless been piecemeal, and, typically, we have *reacted* to recurring crises with temporary and restricted expedients which then tend to become part of the patchwork design called policy. The history of the policies and practices related to the overhead issue itself is but one very restricted example of this tendency. The National Science Foundation action, effective 1 January 1960, allowing up to 20 percent of the total direct costs for indirect cost in new research proposals, is a case in point. Is this simply a continuation of a compromise drift (in 5-percentage-point steps) toward ultimate recognition that indirect costs should be supported in greater measure than they have been? But why not 25 percent this time, and why not a clearer recognition of the direction of the drift and the forthright step of full cost reimbursement immediately?

Rather than concentrate on how much overhead is to be paid and how it is to be figured, is it perhaps not more fruitful to ask whether basic research should indeed be concentrated primarily within our universities? If so, how much should be concentrated there, and what other kinds of institutions might also be involved in such an effort? Should we consider some sort of independent institutes such as the Max Planck Institutes in Germany, which are not directly affiliated with existing universities and whose main objective is the promotion of basic research without the normal interferences introduced by university life? We might also consider some more home-grown examples, such as the Institute for Advanced Study, the Rockefeller Institute, the Carnegie Institution of Washington, and even DuBridge's own California Institute of Technology, as possible models of research-oriented institutes with relatively less emphasis on teaching large masses of students.

Such an arrangement of universities and institutes might well permit the individual scientist to tailor his entire career more precisely to his needs than can be done in the university alone. For example, in his most productive phase the scientist could concentrate all his energies on research in a research institute. At another time he could return to the university and perhaps devote only a part of his time to research while getting some stimulation and change from a program of teaching. At still another time he could move back to full-time research, and perhaps toward the close of his career he might wish to concentrate all or nearly all of his energies in teaching.

Such a plan might well result in capturing "the best of both worlds" for the individual scientist and the institutions. It might also provide additional strength and vigor for our national research effort.

DuBridge suggests "that the long-range scientific strength of America, of the free world, and of all civilization is critically dependent on the vigor and quality of the research activities carried on in American universities." While this is the sort of statement with which one finds it difficult to disagree, the possibility should be considered that the research and scientific strength of America need not be concentrated entirely in American universities.

The pressure on universities, which is likely to increase in future years, to teach more students and to devote themselves to still other functions is so great that the question arises as to how much more of an expansion of research effort is possible, especially among the large university research producers of today. Some universities have already begun to ask basic policy questions about how much expansion of research they can afford in the light of the expansion of teaching that will be required. What kind of balance of teaching and research is desirable? What kind of teaching (graduate and undergraduate) goes together with what kinds of research (large-scale and small-scale, basic and applied)? It is important to reconsider the whole problem of financing our universities, for both teaching and research, and not attempt small-scale, piecemeal solutions.

Further, wherever research is supported, the question of how it should be supported has already been asked many times but still seems to defy a general solution. Perhaps we are now ready to consider abandoning such heavy reliance on the project grant type of support. I am not at all certain that block grants, which are usually offered as a desirable alternative to the present system of short-term grants, constitute a sufficiently broad solution.

It is agreed that full overhead payments will not solve the university's over-all financial problems. While it cannot be denied that such payment might solve one part of that problem, my whole article was based on the thesis that *our approach has already*

been too piecemeal. I disagree with DuBridge's implication that, having solved one problem partially, we can then proceed to the solution of other parts of it while everything else remains constant.

Full reimbursement may bring with it a host of new and unanticipated problems which may make any over-all solution all the more difficult. For example, it might perpetuate the present system of short-term, specific project grant support. Even more important, it might encourage many of the more impoverished schools to accept research projects which bear little relationship to their teaching functions, simply to get on the research "band wagon" and to share the wealth.

In this context it should be noted that poverty (financial or intellectual) is in itself a form of control. Federal agencies with specific missions are more prepared to support certain kinds of research than others, and a university which does not have adequate financial and intellectual resources may be tempted to tailor its research objectives to what is popular and easily supportable. Providing full reimbursement for overhead costs might well prove an additional temptation too strong for such institutions.

It should be unnecessary to repeat the point that "control" may take many forms and affect our spectrum of colleges and universities in a wide variety of ways. Full cost reimbursement will not necessarily result in control of our universities, any more than partial support of research will. But that danger does exist—not because the federal agencies want to exercise that control and not because they are "paying much of the piper's wage," but because some universities, in the process of backing into increasingly larger scale research programs, are abdicating their responsibility to exert control. Further, the rather prevalent attitude that federal subsidies to education are *ipso facto* dangerous because they would lead to control while federal support of research would not have the same effect has tended to obscure our thinking on the whole problem of control.

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Science in the News

"Science" and Advertising: the Federal Trade Commission Is Seeking a Way to Curb Abuses

A few weeks ago the makers of One-A-Day brand vitamin pills published a full-page advertisement in newspapers around the country intended to show why everyone ought to take vitamin pills. The ad was elaborately dignified: the type faces used were small, considering the size of the ad, and not too bold; the brand name was not emphasized; the tone of the piece was not that of a salesman trying to peddle his wares but of a public-spirited organization trying to perform a service by putting the facts about vitamins before the public.

The burden of the argument was that studies by the U.S. Department of Agriculture show that the vitamin content of typical diets is sometimes below the minimum recommended intake; therefore, to be on the safe side, everyone might well spend a few cents a day on a multipurpose vitamin pill, such as One-A-Day brand, to protect himself and his family.

The ad, and others like it promoting other products, presents a peculiar problem. Both Federal Trade Commission officials and nutrition experts in Washington for the AMA convention last week agreed that the ad was scientific hokum. The so-called "minimum" recommended daily intakes established for vitamins, they point out, are actually two or three times the minimum required for good health; therefore the fact that a typical diet may contain less than the established "minimum" of one or more vitamins is no argument for taking vitamin pills; indeed, a person might be receiving one-half to one-third the "minimum" requirements of several vitamins and still not be suffering from vitamin deficiency.

What is disturbing about this particular ad is not that it is misleading. Nearly all over-the-counter (that is, nonprescription) drug advertising is to some extent misleading. (An example is that of an ad that appeared during the Asian flu epidemic of three years ago. A laxative manufacturer advertised: "If you have Asian flu and need a laxative, take ———." The possibilities of this approach are endless; "If you have cancer and need a headache pill, take aspirin.") But whereas the general public can be expected to regard an advertising pitch with a certain amount of skepticism, an advertisement masquerading as a public-service announcement and invoking, by suggestion at least, the authority of some widely respected source—usually, these days, a scientific source—naturally tends to allay such skepticism.

"Public-Service" Advertising

The Federal Trade Commission, which is responsible for protecting the public from misleading advertising, has become interested in this "public-service" type of advertising and is now seeking a good example of which to make a test case. The FTC works under a number of handicaps, one of them being that the burden of proof normally lies entirely on the government. It is one thing to demonstrate that the information on which an advertisement is based does not really prove that it would be a good idea for everyone to take vitamins. It is another and far more difficult thing to prove that there is no reason whatsoever for the ordinary person to take vitamins.

The principal case where the burden of proof shifts from the FTC to the advertiser is that where the public safety is involved. Here the advertiser can be made to show that his product, although possibly worthless, is at least

harmless. With vitamins, it is possible to show that people can damage themselves by overdosing (the usual case is the mother who reasons that if one One-A-Day pill is good for her child, three must be three times as good). But here the blame does not rest with the company, and in any case it can be argued that for every person harmed by vitamin pills there is probably someone else to whom they have been sold who really needed them. There may be grounds for forcing the manufacturer to point out in his advertising not only that one a day is sufficient but that two or more a day are excessive. But that is not the question here.

What the FTC would like to do is to persuade the courts that when advertising claims are presented under the guise of a public-service announcement or as conclusions of an unbiased and widely respected source, the power of the advertising to mislead is so enhanced that the public is entitled to special protection.

The unwritten principle under which the regulations governing the FTC seem to be written is that it is not the business of the government to keep a fool from being separated from his money. One would expect an ordinarily intelligent citizen to realize that the connection between Asian flu and a laxative is not compelling, or to take with a grain of salt vague claims that scientific tests prove that *X* is 67 percent better than non-*X*. But lacking special knowledge of the peculiar meaning of the term *minimum recommended intake*, even a reader with scientific training might be misled by the One-A-Day ad, because of the official government sources quoted and the public-service facade of the advertising.

The Federal Trade Commission considered making a test case of the vitamin ad, or of another ad, similar in tone, for contact lenses. For various reasons it decided against going ahead, and it is still looking for the right test case—one that it can feel almost sure of winning. For to take a case to court and win will establish a principle and make it easier to move against less flagrant offenders, but to take a case to court and lose will make it difficult to try again for several years.

As of the moment, the last word must be granted to a physician at the AMA convention, who pointed out that the vitamin manufacturers really ought to start processing sewage; they have been so successful in selling the American public superfluous vitamin pills, he argues, that American sewage must now contain the world's richest concentration of vitamins.

The Blues versus the Commercials: The Struggle among Opponents of Federal Health Insurance

Although the controversy over health insurance is now centered on the political struggle over the Forand (Social Security) type federal insurance for the aged, an equally interesting struggle among the opponents of federal insurance is going on just behind the scenes.

The adversaries are, on the one hand, the American Hospital Association, Blue Cross, and Blue Shield, together familiarly known as "the Blues," and, on the other hand, the commercial health insurance companies; in the middle is the American Medical Association.

The Blues operate on what they call the social principle: that is, they are committed to the idea of charging everyone, so far as possible, the same price for health insurance in order to make health insurance equally available to everyone. They set a single rate for a community, and every group of employees that is insured with the Blues pays the same rate. This means that a company whose workers are predominantly younger men, a low-risk group, will pay more for their insurance than strict actuarial accounting would require, but it allows the Blues to provide insurance at the same rate to a company whose workers are predominantly middle-aged women, a high-risk group. This, of course, is the way any government insurance scheme would work: equal protection to everyone.

The "Commercials," on the other hand, rate each group separately both in the extent of coverage and in the rate, which allows them to undercut the Blues, sometimes by varying benefits, more often by selling to low-risk groups at a rate below the Blues' community rate. This process, of course, takes the low-risk groups out of the Blues' community rate, and with a disproportionate number of high-rate groups to cover, the Blues must raise their rates. The Commercials say this is good American free enterprise; the Blues say it is in the best interest neither of the public nor of the medical profession.

The Blues are fighting the Commercials with every means at their disposal, including simple harassment. Blue Cross and Blue Shield are sponsored, respectively, by local hospital and medical associations, and it is a common practice—particularly among hospitals, sponsors of the older, more militant, and dominant Blue Cross—to inform patients with commercial insurance that their insurance is no good and to prove it by refusing to honor their policies as guarantees that their bills will be paid.

Aside from the natural inclination of an organization to perpetuate and expand itself, the Blues make a compelling case for their attitude. They assume that the time when health insurance is general throughout the country is not far off. The only realistic question is whether it will be provided by the government or by private organizations. A private insurance system, this view goes, can meet the pressure for government insurance only by providing substantially the benefits that could be expected under government insurance but by doing so in the American tradition of keeping government involvement in private affairs to a minimum.

For several reasons—most notably, to be able to make the same insurance available to everyone at the lowest price, as the government would—the first order of business, as the Blues see it, is to see that commercial insurance companies get out of the health insurance field. After this, the Blues see the way clear to establish a national health insurance system which would essentially parallel what could be provided by a government system under Social Security, but with control in the hands of the medical profession and the hospitals rather than government officials, although the government, of course, would have regulatory powers, as it does with other legal monopolies. Membership in the system would probably have to be just as compulsory as it would be under a government Social Security system, although this notion is still the rankest of all heresies within the AMA.

Position of the AMA

The ideas outlined above, based on an interview with a high-ranking official of the Blues, place the AMA in an awkward position. The AMA is dedicated to free enterprise, but it is being asked to cooperate in forcing free enterprise out of the health insurance

field. It is against compulsory health insurance, but it is being asked to cooperate in a venture which may require compulsory membership in order to succeed. It is against getting the government involved in medicine, but it is being asked to support the establishment of a government-chartered monopoly on health insurance, with the substantial degree of government regulation such chartering implies. And it is against socialism in any form, but it is being asked to give considerable support to a bit of socialism right under its own wing. To all this the Blues answer simply that either the medical profession will take the lead in solving the problem or the government will; that before rejecting one road as objectionable the AMA ought to consider how objectionable the alternative might be.

The issue provided the most strenuous debate at the Association's convention in Washington last week. The Board of Trustees produced a recommendation that the AMA, in effect, ought to get everyone in the health insurance field, including the Commercials, together to try to see what can be done. The Blues rejected this proposal out of hand. Their only purpose in getting together with the Commercials, they said bluntly, would be to talk about how quickly the Commercials could get out of the health insurance field.

The reference committee on resolutions affecting insurance turned down the Board of Trustees' report and produced a compromise resolution committing the AMA to working with the Blues in taking the lead on health insurance but including another clause, in vaguer language, promising to continue working separately with the Commercials. When the resolution came before the full AMA House of Delegates there was a brief floor fight intended to restore the sense of the Board of Trustees' approach, but the compromise resolution was upheld by a 2 to 1 margin.

When a delegate asked the chairman if he didn't interpret this compromise as in effect writing off the Commercials, the chairman said he didn't think it should be interpreted quite that way. But the representatives of the Blues, although they are still far from having won the whole-hearted support of the AMA leadership that they would like, were thoroughly satisfied. The Blues were elated, and the Commercials were blue.—H.M.

News Notes

State Department Restrictions on Meeting Participation Questioned

The Federation of American Scientists has asked the Department of State to clarify its stand limiting the participation of government scientists in international meetings. Under an ill-defined policy, the department has in some cases forbidden scientists employed by the government to go to international meetings attended by Communist Chinese, on the ground that participation might imply a weakening of this country's policy of not recognizing mainland China.

However, the department's ruling has been unevenly applied. Not long ago Naval Research Laboratory men took part in international conferences attended by both East German and Communist Chinese representatives, yet in May three Public Health Service officers, in Moscow for bilateral meetings, were not allowed to participate in a multilateral symposium because Chinese Communists were to be present.

Recently FAS asked State for clarification. The action was spurred by reports that scientists at the National Institutes of Health believe they may not be permitted to attend next summer's international biochemistry meeting in Moscow.

After informal discussion of the Moscow meeting with officials of the National Science Foundation, the U.S. Public Health Service, and the State Department, the FAS on 31 October sent a letter to the Secretary of State that asked: "Will [government] scientists be prohibited by your Department, or by Departmental advice to other Government agencies, from attending the 1961 Moscow meeting?"

Since it has been suggested that the present policy makes it possible for the Communist Chinese to control our government scientists' participation in international scientific congresses, it is hoped that the State Department will issue a prompt and favorable reply.

Atmospheric Research Center To Have Colorado Headquarters

Selection of Table Mountain, near Boulder, Colo., as the headquarters site for the National Center for Atmospheric Research has been announced by the National Science Foundation and the University Corporation for Atmospheric Research. The latter group, which will manage the new center for the NSF, is composed of representatives of the following 14 colleges and universities with degree-granting programs in meteorology: Arizona, Chicago, Cornell, Florida State, Johns Hopkins, M.I.T., Michigan, New York University, Pennsylvania State, Saint Louis, Texas A. and M., U.C.L.A., Washington (Seattle), and Wisconsin.

With Walter Orr Roberts as director, the center is being established to assist universities and other research institutions through cooperative basic research, to increase knowledge and understanding of the atmosphere and of the physical processes that govern its behavior. The center will, as its programs mature, be particularly concerned with fundamental studies on a planetary scale, such as interpreting radiation effects of changes in world cloud cover, understanding the development of planetary wave systems manifested in the meanderings of the jet stream, and analyzing the association between northern and southern hemispheric circulation on a year-to-year time scale. Roberts says:

"The national center will be primarily an intellectual enterprise, devoted to fundamental research on broad atmospheric problems. It will serve as a coordinating center for a wide-ranging network of such investigations. This effort can be expected to develop a much more comprehensive understanding of weather and other phenomena than has ever been possible through isolated research."

Approval of the site came after 2 years of work by a UCAR site selection committee. Among the advantages offered by Table Mountain, the committee noted that Boulder is a highly desirable location for study of hail, thunderstorms, tornadoes, and associated squall line phenomena; that it is a desirable location for study of jet streams and of weather in the belt of maximum westerlies; that nearby mountain areas produce local weather phenomena that may materially affect weather conditions throughout the Great Plains and even more extensively over the United States; that high ionospheric layers and atmospheric airglow phenomena can be readily observed and studied at this site; that generally excellent flying conditions prevail; and that Boulder is centrally located with respect to the distribution of research centers now engaged in atmospheric

research and training. However, because no single location can provide access to all phenomena likely to be studied, field stations and supplementary research facilities may later be set up elsewhere in the United States by the corporation.

Health Science Manpower Survey To Be Conducted in Pittsburgh

A survey of the nation's needs in health science manpower has been launched by the Federation of American Societies for Experimental Biology. The survey has been undertaken because of the underlying need for more personnel in the health sciences. Objectives of the study will include an assessment of the present supply and demand for scientists in each of about 12 basic health fields, projections of such supply and demand for the next 10 to 20 years, and recommendations on how to meet the increased demand for such scientists.

John T. Cowles, director of educational planning for the health professions at the University of Pittsburgh, is director of the project. Lowell S. Levin has joined Cowles's staff in Pittsburgh to serve as full-time associate director.

A planning phase, which should be completed by June 1961, will focus at first on the review of manpower problems in microbiology. The over-all project, which will require from 3 to 5 years to complete, will be expanded to include most, if not all, of the health science fields.

The pilot phase of the project is being supported by an $87,000 grant from the National Institutes of Health. The University of Pittsburgh has been selected as the project site because of its electronic computer facilities and the availability of qualified personnel.

AIBS Launches Communications Project

The Biological Sciences Communications Project, a new activity of the American Institute of Biological Sciences, has been awarded a $151,200 grant by the National Science Foundation. "The mission of the new Project is to study carefully all steps in the flow of information from the person who produces it—usually a scientist—to the person who uses it," says Charles W. Shilling, project director. Shilling joins AIBS from the Atomic Energy Commission, where for 5 years he served as deputy director of the Division of Biology and Medicine.

As a first step, the over-all problem of acquiring, indexing, storing, and retrieving printed scientific literature will be reviewed. Science communication as it is influenced by the training of scientists, from grade school through university and graduate levels, will be considered, as will the needs of teachers, administrators, persons in public relations work, and science writers. The new group will also appraise the effectiveness of what has been called the "people-to-people" approach in science—such programs as "science emphasis week" (observed at many colleges), summer laboratory assignments, and the Visiting Biologists Program. At the international level, the project will consider developing a practical method for obtaining and disseminating, in this country, valuable information on foreign research acquired by U.S. scientists visiting in other nations.

The Advisory Committee for the Communications Project, which has been developing plans for more than a year, will meet four times a year. Spokesmen for federal, university, and industrial groups will brief the committee on communications activities of interest in the biological sciences. The next meeting is scheduled for 26–27 January 1961.

Pacific Science Congress Plans Well Advanced

The National Academy of Sciences, the Bernice Pauahi Bishop Museum, and the University of Hawaii are hosts to the tenth Pacific Science Congress, to be held at the university from 21 August to 6 September 1961. The sponsoring organization for these congresses is the Pacific Science Association, an international and nongovernmental organization whose membership includes 47 countries that rim the Pacific and other countries with interests in the Pacific. Each member country is represented through its principal scientific organization.

The program for the tenth congress includes 124 symposia, on agricultural sciences, biological sciences, conservation, forestry, geography, geophysical sciences, public health and medical sciences, and scientific information. The program will also include sessions for contributed papers and reports of standing committees of the Pacific Science Association. Among the special symposia are three entitled, respectively, "Man's Place in the Island Ecosystem" (sponsored by UNESCO), "The Galapagos Islands," and "Science Museums in the Pacific Area."

Approximately 1000 U.S. and foreign scientists have been invited to present papers. Some travel funds are available (see "Grants, Fellowships, and Awards").

Other prospective features of the congress are exhibits, visits to research vessels, and a program of science films. A display showing the United States' participation in geographical exploration and in the surveying and mapping of the Pacific basin during the period 1783–1899 is being arranged by the National Archives.

Countries that operate research ships in the Pacific have been invited to have one or more of their vessels put into port at Honolulu at the time of the congress. Whenever possible, arrangements will be made for participants to go aboard these vessels to see the specialized equipment and discuss the research programs.

Inquiries about the congress should be addressed to Harold J. Coolidge, Secretary-General, 10th Pacific Science Congress, Bishop Museum, Honolulu 17, Hawaii.

News Briefs

WHO budget proposal. The Executive Board of the World Health Organization has recommended a budget of $21,576,480 to finance the organization's operations in 1962. The figure represents an increase of $1,775,752, or 8.97 percent, over the figure for 1961.

It has also been announced that two-thirds of WHO's member states have voted to increase the Executive Board's membership from 18 to 24 members. The board's sessions were concluded recently in Geneva, WHO's headquarters site. The recommendations must now go to the World Health Assembly, which meets in New Delhi in February.

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Erythropoietin available. A limited supply of an erythropoietin preparation from sheep plasma, with a potency of approximately 10 cobalt units per milligram, is available for distribution. The material is to be used only for

physiological experimentation at the laboratory-animal level and for chemical studies. In view of the state of purity of the product and the limitation of the supply, it is not intended for use in clinical investigation. Requests for this material, accompanied by a specific statement describing the proposed experiments, should be addressed to Dr. James Stengle, Executive Secretary, Hematology Study Section, Division of Research Grants, National Institutes of Health, Bethesda 14, Md., or to Dr. Kenneth Brinkhous, Chairman, Hematology Study Section, University of North Carolina, Chapel Hill, N.C.

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Starting salaries. The following are monthly salaries accepted by this year's college graduates: chemical engineering, $525; civil engineering, $489; electrical engineering, $536; mechanical engineering, $519; natural sciences, $460; physical sciences, $521. This information, as well as a further detailed analysis of the survey, appears in the October issue of the *Journal of College Placement*.

* * *

Radiation protection course. The fourth advanced course in the principles of radiation protection will be held in the United Kingdom Atomic Energy Authority's Harwell Reactor School from 19 April to 18 July, 1961. The course is intended for graduates or persons of graduate level either entering or already working in the field of radiological health and safety. A high academic standard is set, but the program will begin with first principles and will not require previous specialized knowledge.

Most of the students attending these courses have come from the UKAEA; a small number have come from other organizations in the United Kingdom and from countries in Europe and Asia. Application forms and further details can be obtained from the Manager, Reactor School, Atomic Energy Research Establishment, Harwell, Berkshire, England.

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NIH grant. The National Institutes of Health, principal research arm of the Public Health Service, awarded 11,743 grants totaling $229,505,503 for research and for construction of research facilities in nonfederal institutions during the fiscal year ended 30 June 1960. Detailed information on the nature, distribution, and amounts of these awards is contained in a 445-page summary just issued: *Public Health Service Grants and Awards by the National Institutes of Health, Fiscal Year 1960*, Part I (available from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C., at $1.25 per copy).

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Lemur colony. John Buettner-Janusch, an anthropologist at Yale University, recently brought 25 lemurs from the Malagasy Republic (Madagascar) to establish a colony. There has been almost no research on the lemur, now threatened with extinction, although it has lived for many millions of years, seemingly in defiance of evolution. Among the factors to be studied in the new colony is the extremely high clotting rate of lemur blood. The blood tends to clot faster than it can be withdrawn through a hypodermic needle.

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Geological research results. A comprehensive summary of recent geologic findings by the U.S. Geological Survey has been published as two separate "chapters" of *Professional Paper 400*. Chapter A, which has 138 pages and four illustrations, is a synopsis of a wide variety of geologic studies and was prepared under the direction of Vincent E. McKelvey by members of the Survey's Geologic Division. Chapter B, with 515 pages and 303 illustrations, consists of 232 papers, averaging about 1000 words in length, by individual authors. Publication of these volumes is an experiment aimed at meeting widespread public and professional demand for important research results at the earliest possible date. Copies of professional papers *400-A* and *400-B* can be obtained from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C., at $1 and $4.25, respectively.

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Harvard program in medicine. Harvard University has announced a $58 million program in medicine, for which it already has $21,627,838 in gifts or pledges. The objective is to strengthen the faculty of medicine by providing for more full-time appointments, for increased salaries, and for more unrestricted capital to develop promising areas in medical teaching and research. The only building included in the program is the new Francis A. Countway Library of Medicine, which will house the Harvard and Boston Medical Libraries.

Grants, Fellowships, and Awards

Biology. Applications are invited for two $5000 Turtox scholarships established by the General Biological Supply House, Inc., to support graduate students who wish to study for the doctoral degree in botany, zoology, or biology in preparation for careers in teaching and research. Applications, which must be returned by *1 February 1961*, may be obtained from the chairman of the award committee, Professor Frank A. Brown, Jr., Department of Biological Sciences, Northwestern University, Evanston, Ill.

Medicinal chemistry. The Medical College of Virginia has received a grant from the National Institutes of Health to support training in medicinal chemistry. The new grant, the first of its kind, will provide fellowships for graduate students who select their major in the department of chemistry and pharmaceutical chemistry, affording them not only training in fundamental chemistry but also an opportunity to become familiar with the biologist's approach to research in the medical sciences.

The fellowship allowances are generous, and any student with a bachelor's degree in science may apply. Requests for application forms or further information should be sent to: The Dean, School of Graduate Studies, Medical College of Virginia, Richmond 19, Va.

Pharmacology. Wyeth Laboratories of Philadelphia has established the $2500 Torald Sollmann Award in Pharmacology, which will be awarded for significant contribution to the field. The members of the council of the American Society for Pharmacology and Experimental Therapeutics will constitute the Torald Sollmann Award Committee; the senior councilor will be the committee chairman.

Nominations for the award may be made by any member of the society, but no member may nominate more than one candidate. Nominations may also be accepted by the committee from members of other scientific associations, both domestic and foreign. Nominations, manuscripts or reprints of the candidate's publications covering the work for which the nomination is made, and a brief biographical sketch of the candidate must be submitted in triplicate to the secretary of the society, to be forwarded to the chairman of the award committee not later than *15 January*.

Presentation of the award, which in-

cludes a medal, will be made at a meeting of the society. At the discretion of the society president, the recipient is to deliver a Sollmann Oration covering his major contribution. Traveling expenses of the recipient to the meeting at which the award is presented will be paid by Wyeth, and Wyeth will also assume the nominal expenses of the award committee in selecting the recipient. Initially, an award at intervals of 3 years is contemplated. The first Torald Sollmann Award in Pharmacology will be given at the 1961 Fall Meeting of the society in Rochester.

Protozoology, travel. The first International Conference on Protozoology, arranged under the auspices of the Czechoslovak Academy of Sciences, will be held jointly with the 13th meeting of the Society of Protozoologists in Prague, 22–30 August 1961. Financial assistance for travel to the Prague meeting is available for a limited number of qualified U.S. scientists who wish to present papers. Applicants should write to: William Trager, President, Society of Protozoologists, The Rockefeller Institute, 66th St. and York Ave., New York 21, N.Y. The letter, which must describe the general nature of the proposed paper, should include a brief summary of the applicant's career. Applications must be received by *3 January 1961*.

Science teaching. The National Science Foundation has announced the award of grants totaling about $9 million to 43 colleges and universities to support Academic Year Institutes for science and mathematics teachers. This will be the sixth year of this program, which was established to help teachers improve their knowledge of subject matter through a year's advanced study on a full-time basis. Special emphasis is placed in these institutes on the newer developments in science and mathematics.

Approximately 1500 experienced junior and senior high school teachers will attend the institutes during the 1961–62 academic year. Each teacher will pursue a course of study in science or mathematics planned especially for him and conducted by scientists noted both for competence in their fields and for skill in presentation.

About 75 teachers from liberal arts colleges, teachers colleges, and junior colleges will also be included in the program. They will be chosen from those who have primary responsibilities for the training of prospective science and mathematics teachers in their own institutions.

The foundation grants will provide stipends of $3000 for each participant, with additional allowances for dependents, books, and travel. Some institutes will provide an additional summer training program to enable teachers more easily to fulfill graduate degree requirements. Supplementary allowances will be provided for teachers participating in this extended program.

Support of these institutes by the National Science Foundation encourages colleges and universities to offer special programs in subject-matter instruction for teachers already in service, as well as providing financial support for teachers so that they can undertake advanced studies.

The NSF has prepared a list of the institutions receiving grants and names the institute directors. Information and application forms should be obtained from the directors of the individual institutes, *not* from the National Science Foundation. The application deadline is *20 January 1961*.

Sterility. The $500 Carl G. Hartman grant-in-aid of the American Society for the Study of Sterility is available for 1961. Applications should be sent by *1 February* to the secretary of the society's awards committee, Dr. Anna L. Southam, 620 W. 168 St., New York 32, N.Y. The application must be accompanied by five copies of a brief outline of the research project for which the grant is being sought.

Travel, Pacific congress. The National Science Foundation, along with other government agencies, is cooperating with the Pacific Science Board of the National Academy of Sciences–National Research Council in the support of travel of American scientists to the tenth Pacific Science Congress, to be held in Honolulu from 21 August to 6 September 1961. Each grant will be limited to the cost of a round-trip ticket (air tourist) to Honolulu from the home city. Preference will be given to applicants who have been invited to participate in scheduled symposia in the physical, biological, and social sciences; in fields concerned with research in the Antarctic; and in fields concerned with the dissemination of information among scientists.

Letters of application (no special blank is necessary) should be sent *before 15 January* to the Pacific Science Board, National Academy of Sciences–National Research Council, 2101 Constitution Ave., NW, Washington 25, D.C.

Scientists in the News

Lloyd V. Berkner, for 10 years president of Associated Universities, Inc., New York, has resigned to accept the presidency of the Graduate Research Center, Dallas, Tex. He is succeeded by **Leland J. Haworth**, vice president of AUI and director of the Brookhaven National Laboratory, who has been named acting president. During Berkner's tenure, Associated Universities managed the Brookhaven National Laboratory, Upton, N.Y., under contract with the Atomic Energy Commission, and developed the National Radio Astronomy Observatory at Green Bank, W. Va., under contract with the National Science Foundation.

In announcing Berkner's appointment, J. E. Jonsson, chairman of the Dallas center's board of trustees, said that the trustees foresee an expanding scope of activities for the center and development of large-scale research facilities in the Southwest. This development will be closely related to the needs of important colleges and universities in Texas and neighboring states, with a view to greatly extending opportunities for graduate education and postdoctoral research.

Ernest M. Allen, chief of the Division of Research Grants and associate director for research grants at the National Institutes of Health, has been given full-time staff responsibility as associate director, according to Surgeon General Leroy E. Burney. The reassignment, recommended by NIH director James A. Shannon, was brought about by the rapid growth and complexity of research grant activities of the institutes.

Operating responsibilities for the Division of Research Grants, the coordinating agency for NIH's program of grants and awards in the field of medical and allied research, will be carried out by **Dale R. Lindsay**, former assistant chief of the division, who has been appointed chief. He will be assisted by **Clinton C. Powell**, the new deputy chief. Powell was formerly assistant chief of the division's branch for clinical research grants.

Estill I. Green, executive vice president of Bell Telephone Laboratories, retired on 30 November after a distinguished career of 39 years with the Bell System. He has been granted more than 70 patents for his inventions and is the author of many articles on

Estill I. Green

scientific subjects and on the management and evaluation of technical personnel.

He began his career in telephone communication in 1921, in the American Telephone and Telegraph Company's development and research department. There he engaged in transmission studies and the planning of new transmission developments, especially the carrier telephone and telegraph systems that were just then getting started. He took a leading part in the planning of the coaxial system, which now provides a major part of the Bell System's long-distance telephone and television network.

During World War II he took over broad responsibilities for developing apparatus to test radar systems. Under his general direction some 250 designs of radar test gear were developed.

Green received the A.B. degree from Westminster College, Fulton, Mo., in 1915. After a year of graduate study at the University of Chicago he returned to Westminster as professor of Greek. On his discharge from military service in 1919 he entered Harvard Engineering School and was graduated (summa cum laude) in 1921 with the B.S. degree in electrical engineering. He joined A.T.and T. immediately after graduation.

The National Science Foundation has announced eight new staff appointments.

F. Philips Pike, professor of chemical engineering at North Carolina State College, has been appointed program director for engineering sciences in the Mathematical, Physical and Engineering Sciences Division. Since February 1960, Pike has been assistant program director for engineering sciences.

In the Division of Scientific Personnel and Education, **Charles A. Whitmer** has been appointed head of the Course Content Improvement Section, and **Conrad E. Ronneberg**, program director for secondary school programs, Special Projects in Science Education Section. Whitmer is on leave from Rutgers University. Ronneberg is on leave from Denison University, where he is senior professor of chemistry.

Harve J. Carlson, who has been with the foundation since 1959 as program director for biological facilities, has been appointed deputy assistant director in the Division of Biological and Medical Sciences.

The other four NSF appointments are also in the Division of Biological and Medical Sciences.

Dean R. Parker has been named program director for genetic biology. He is on leave from the University of California, Riverside, where he is professor of biology and chairman of the Division of Life Sciences.

Daniel Billen has been appointed program director for metabolic biology. He is on leave from the M. D. Anderson Hospital and Tumor Institute of the University of Texas, Houston, where he has been chief of the section on microbiology.

Paul J. Kramer has been appointed program director for regulatory biology. He has taught since 1931 at Duke University, where he holds the James B. Duke professorship in botany.

Dixy Lee Ray has been appointed consultant and will advise the foundation in formulating a support program in biological oceanography. She is associate professor of zoology at the University of Washington.

Harry Hoogstraal of the U.S. Naval Medical Research Unit No. 3 in Cairo, Egypt, has been elected an honorary fellow of the Egyptian Public Health Association "for his invaluable contributions in the field of public health." At the presentation ceremony, A. M. Kamal, president of the association, stressed the considerable influence that Hoogstraal's research has had in the fields of human and veterinary medicine in Africa and Asia and his long, friendly cooperation with the association and with the High Institute of Public Health in Alexandria. The occasion was also a farewell to Hoogstraal, who will be spending many months of the next few years in the Sudan studying the epidemiology of kala-azar.

George W. Corner, historian of the Rockefeller Institute, New York, and director emeritus of the department of embryology, Carnegie Institution of Washington, has been appointed executive officer of the American Philosophical Society, Philadelphia. He will retain a connection with the Rockefeller Institute as visiting professor.

The University of Rochester has recognized two outstanding members of its faculty by naming them "distinguished senior professors," a newly created title. The two men honored were **Wallace O. Fenn**, chairman emeritus of the physiology department and professor of physiology, and **W. Albert Noyes, Jr.**, Charles Houghton professor of chemistry.

Henry L. Bockus, internationally known gastroenterologist and emeritus professor of medicine and gastroenterology at the University of Pennsylvania's Graduate School of Medicine, was recently honored by the dedication in his name of the university's new, five-story Henry L. Bockus Research Laboratories.

Richard A. Barkley has joined the staff of the Honolulu Biological Laboratory, U.S. Bureau of Commercial Fisheries, as chief of oceanography investigations.

Walter H. Maloney has been named clinical professor of laryngology and bronchoesophagology at the Temple University School of Medicine. He will be a member of the Chevalier Jackson Clinic. Maloney joins Temple from Western Reserve University, where he served as associate professor and director of otolaryngology. He also held the post of director of the otolaryngology division at the University Hospitals of Cleveland.

J. Harold Upton Brown, formerly professor of physiology at Emory University, has been appointed executive secretary of the Physiology Training Committee in the Division of General Medical Sciences of the National Institutes of Health, Bethesda, Md.

Mildred Horton will retire on 15 December after 14 years as executive secretary of the American Home Economics Association, Washington, D.C. She will make her home in Houston, Tex. Her successor is **A. June Bricker**, former director of the Field and Community Health Bureau of the Metropolitan Life Insurance Company, New York.

J. Walter Wilson, Frank L. Day professor of biology at Brown University, has relinquished the chairmanship of the biology department. He is succeeded as chairman by **Mac V. Edds, Jr.**, a professor in the department.

Robert S. Mulliken, Ernest de Witt Burton distinguished service professor in the physics department at the University of Chicago, visited Sweden in September to receive an honorary doctoral degree, in recognition of his work on molecular spectra, from the University of Stockholm on the occasion of its inauguration as a Royal University.

David Middleton, a consulting physicist with industry, the armed services, and several university laboratories, has been appointed adjunct professor of electrical engineering at Columbia University, where he will present a series of lectures during the fall and winter on advanced methods in statistical communication theory.

William W. Walton has recently been appointed chief of the organic building materials section of the Building Research Division at the National Bureau of Standards. Formerly he was chief of the surface chemistry section in the Chemistry Division. Walton succeeds **H. R. Snoke**, who recently retired.

Paul F. Hahn has been appointed chief of the Office of Extramural Grants in the Division of Radiological Health of the U.S. Public Health Service, Washington, D.C.

Holbrook MacNeille, professor and chairman of the department of mathematics at Washington University, has been appointed professor and head of the department of mathematics at Case Institute of Technology. MacNeille's research has been in the areas of partially ordered sets, lattice theory, and Boolean algebra. Since 1955 he has been concerned with educational television.

Ellis B. Page, formerly at Eastern Michigan University, has accepted the post of dean of the College of Education and professor of education and psychology at Texas Woman's (State) University, Denton.

Paul W. Oman, chief of the Insect Identification and Parasite Introduction Research Branch, Entomology Research Division, U.S. Department of Agriculture, Beltsville, Md., is at present serving as director of the USDA's Far Eastern Regional Research Office in New Delhi, India. He has responsibility for the development and coordination of a research grant program utilizing funds authorized under P.L. 480. The research that is being conducted will be of interest both to the United States and to countries in Asia and the Far East.

Irving L. Schwartz, senior scientist and attending physician at Brookhaven National Laboratory, Upton, N.Y., has been appointed Joseph Eichberg professor of physiology and chairman of the department in the College of Medicine, University of Cincinnati. He succeeds **William D. Lotspeich**, who left the university in July 1959.

Frederick D. McCandless, associate professor of psychiatry at the Albany Medical College of Union University, will head a new subdepartment of behavioral sciences that has been created within the college's department of psychiatry. He will administer a newly established program in the behavioral sciences, to be presented to students during their first 2 years of medical training, beginning next September.

In recognition of the "long and devoted service" of **Herman Beerman**, secretary-treasurer of the Society for Investigative Dermatology, the board of directors of the society has established an annual Herman Beerman lectureship. It is proposed that this lecture be given by a distinguished scholar at the annual scientific session of the society and that the lecturer be given an honorarium.

Eugene Ackerman, professor of biophysics at Pennsylvania State University, has joined the staff of the Mayo Clinic, Rochester, Minn., in the section of biophysics. He is also associate professor of biophysics in the Mayo Foundation of the University of Minnesota in Rochester.

Arthur W. Thomas, who became emeritus professor of chemistry at Columbia University in July 1959, has continued since then in part-time service. He is working under the auspices of the Institute of Nutrition Sciences in Columbia's School of Public Health, where he is serving as lecturer in public health and in administrative medicine and is also continuing his course on the analytical chemistry of foods and vitamins.

W. H. Allaway has been appointed director of the U.S. Plant, Soil, and Nutrition Laboratory, Ithaca, N.Y., to succeed **K. C. Beeson**. Beeson has accepted an assignment with the International Cooperation Administration in the Sudan. Allaway, who has been assistant director of the division since 1957, will transfer to Ithaca from the division office at Beltsville, Md., shortly after the first of the year.

David Landy has been appointed associate professor of anthropology in the University of Pittsburgh's Graduate School of Public Health, with a joint appointment in the department of anthropology. He will organize a program of teaching and research in the social sciences in the various areas of public health and plans to teach a course in primitive and folk medicine in the academic department.

Recent Deaths

Mason H. Campbell, Providence, R.I.; 66; retired dean of the College of Agriculture of the University of Rhode Island and an authority on the dairy industry; 30 Nov.

Gerald R. O'Brien, New York, N.Y.; 64; plastic surgeon and otolaryngologist, who was chief plastic surgeon at the Kings County Hospital Center and clinical professor of surgery at the State University of New York College of Medicine, Downstate Medical Center, Brooklyn; 1 Dec.

Ewald Rohrmann, Indianapolis, Ind.; 48; chemist and staff assistant to the executive director of scientific research for Eli Lilly and Company; had been engaged in research for Lilly for 20 years and had made significant contributions in the areas of sex hormones, antihistamines, and penicillin; 24 Oct.

Leonard G. Worley, Manhasset, N.Y.; 55; professor of biology at Brooklyn College; a leader in research on the Golgi bodies; 7 Nov.

Book Reviews

Presidential Transitions. Laurin L. Henry. Brookings Institution, Washington, D.C., 1960. 738 pp. $7.50.

To understand the burden of this book the prospective reader has only to use (very slightly) his imagination concerning a time and events which he will realize are already familiar to him in a general way. During the months of 1960 prior to November, anyone who wanted to frighten a company of friends out of their wits might well have done it very simply. He could merely have recited to them, as true, a set of political circumstances and a sequence of happenings—the circumstances factual, the happenings entirely possible. The exposition might have run something like this:

The incumbent President of the United States could not succeed himself because, for the first time, the 22nd amendment prevented it. He was, anyway, an aging man, certain to be displaced by one belonging to a new generation. Even if the new President belonged to the same party, there would be changes in policy and an overhaul of top-level personnel. If he belonged to the other party, these changes would be drastic; and the personnel would be almost entirely replaced.

In anticipation of such a governmental revolution, the more than 1200 policymakers and administrators really responsible for making decisions and running the government were leaving as rapidly as they could find positions elsewhere. By election time the government would be manned by those who had not been able to find other jobs; the least effective, political jobholders would be left in charge. It would be months before the incoming appointees would take their places, and then further time would be required for these inexperienced replacements to learn their duties.

The general result would be that both before and after election, and even after inauguration of the new administration, the federal establishment would be seriously enfeebled and would be utterly incapable of meeting any demands of an unusual sort made upon it. And a crisis of major proportions would either paralyze it or cause convulsive decisions to be made which might be disastrous.

Only one sector of the government would have conserved its capability: the military. The generals and admirals would be especially vigilant because of the known incapacity of the civil branches.

The legislative branch would have been undergoing a shake-up even more severe than that of the executive. All of the representatives and one-third of the senators would have been affected by the election. Many would be new even if there was no party overturn. They would meet in January and would need some time to organize and to establish working relations with the new executive. If the Congress should have a majority belonging to another party than the President, these problems would be magnified. Whatever the results of the election, it would be weeks, or even months, before effective legislative work could be done.

To go on, it would take no imagination at all to consider that through several years there had been growing a quarrel with another great power whose strength was increasing and whose belligerence grew with its strength. This quarrel had reached crisis proportions several times. There was, in consequence, an armament race, and the armaments were of novel and horrendous sorts. It was realized that both sides of the conflict would lose by an outbreak of actual hostilities; hence, hostilities must be avoided by diplomacy, by compromise, by reaching an eventual accommodation. The existence of the nation into the future was involved in this process. No more delicate, difficult, or momentous task had ever fallen to a set of officials in Washington.

But at the moment of most complete disorganization, with one administration practically dismantled, the President without prestige, and the succeeding administration not yet installed, the rival great power, sensing its opportunity, would deliberately precipitate a new crisis, perhaps involving one of the trouble spots already the subject of controversy. The rival would have no difficulty working this crisis up to dangerous proportions, so that, just before or just after the election, the rival would come into a position to demand that we surrender to its views and claims on penalty of resort to force.

The choice of policies open to the United States would be these: The American position could be abandoned; negotiation, with a view to compromise, could be sought; or the enemy could be defied. But, in fact, the executive would be quite incapable of negotiating. The other nation would not believe it capable of reaching a decision that would be respected by the successor administration which might even be installed by another party. But the other alternatives would be almost as difficult. If there should be a surrender, the nation would be irreparably injured. If the blackmail should be defied, the nation would be incapable of putting in train the immense crash program required to meet the threat of war.

It could be guessed that the paralysis would most likely end in surrender, simply from incapacity to reach any other solution. But the generals and admirals, seeing this decision in the making, might ask themselves whether their duty to the nation was not greater than their duty to the institutions of democracy and to the Constitution in which these institutions were embodied. They might very well feel compelled to fill the power vacuum by taking measures of their own. After all, the means within their control would be sufficient to enforce any decision they might jointly reach. So the Republic, for the first time in its history, might find itself a military dictatorship.

There were plenty of those in a position to assess the situation to whom this imagined account would seem not too unlikely. If it did not actually happen, the nation would be fortunate to have come through a period of grave peril without disaster. But they would feel that the nation ought to have learned something from its narrow escape. In looking around for revisions of our national institutions which would make it unnecessary ever again to un-

dergo a similar ordeal, they would inquire whether the peril of 1960 was unique and, therefore, perhaps unlikely to happen again.

The answer to this would require two sorts of exploration. The first might be to ask whether transition periods in the past have produced perils of a similar sort; the second would be to ask whether events have occurred—inventions made, processes perfected, alignments arranged—which make present and future interludes peculiarly dangerous.

Looking to modern developments, the inquirers would have to seek no further than their newspapers; they would, in fact, have been looking with increasing anxiety at reports of such developments published in them during the last decade.

It is the purpose of Laurin Henry's book to trace the events of the past few transition periods. Of these he recounts in detail those which have occurred since Wilson took over from Taft in 1913. Each transition has seen the government enfeebled; but the more recent have been more serious because the dangers of enfeeblement have grown. Not only domestic problems require clear policy determinations; foreign affairs demand unremitting attention. The instance of Cuba's escape from the American family and its entrance into agreements with hostile powers is an illustration of what can happen just from inattention at the end of an 8-year administration. Such lapses possess more dangers at present than they have in the past; but we tend to forget that the problem is no different in kind—only in intensity—from those of other transition periods. For this forgetfulness Henry's book is a good corrective.

When Wilson took over from Taft, Henry reminds us, recognition of the new Chinese Republic was pending; settlement of the long dispute with Colombia over the acquisition of the Canal Zone waited; a treaty of friendship with Russia was expiring; and there was an acute dispute with Great Britain over canal tolls when the new waterway should open. This was not all. Domestically, the new parcel-post system had to be organized; railroad mergers were being fought; and a reorganization of the army was in process. These and other matters simply had to be suspended indefinitely as the old term ran out and the new one waited to begin.

When Wilson gave way to Harding in 1921, the situation was even worse. Postwar adjustments had yet to be made. Before the war ended, the Republicans had come into control in Congress, and peace had not even been declared. The dismantling of the war organization had been precipitate, and the chaos in every part of the government was paralyzing. The new President was peculiarly a creature of the politicians in his party, and this ensured the complete replacement of experienced civil servants with amateurs whose qualifications were purely political. It was a long time before a measure of competence was regained—except, perhaps, in the Department of State, where the effective Charles Evans Hughes was in charge.

When next the Republicans gave way to the Democrats, the frightening events of 1933 were in progress. The depression of the past few years had turned ugly indeed and by then had reached the upper levels of finance and business. When the new President took office, the banks were closed, unemployment had reached an incredible proportion of the working force, conservative farmers in the West were staging revolutionary riots, foreign nations were refusing to meet their debt payments, a world economic conference was about to meet, and disarmament negotiations at Geneva waited for new impetus. The outgoing President contended then, and has contended since, that it was the uncertainties of transition which made it impossible to control the degeneration that affected all these public events. He too had a hostile Congress to contend with; and no policy determinations could be made.

To go on, the takeover of the Eisenhower administration after 20 Democratic years had to be carried out in equally difficult circumstances. There was, among other things, an unfinished war in Korea. But there were many other policies on which it was assumed that reversal or severe modification could be expected. And there were now a much larger number of policy positions where replacements could be expected. The responsibilities of government both at home and abroad had immensely increased, and it was correspondingly more serious that the old controls should slip before the new ones could become effective. But there was no remission of the rule of displacement.

It was during this last transition that competent observers were confirmed in an already strong opinion that some way would have to be found to modify the disruptions of change. The nation could no longer tolerate a period of a year or more during which policy decisions were in abeyance and, no matter how serious the crisis, there was no competent authority to meet it. Why this feeling spread is adequately explained in this book. The nation had come into increased responsibilities; and it existed in a world whose dangers were infinitely greater than they had ever been before.

At the end of Henry's book there is a section concerning the future. In this he summarizes the suggestions offered by students for modifying the dangers of the interregnum. There are, of course, some difficulties that cannot be overcome without infringing democratic principles. The election that has taken place had as its purpose the choosing of a President and a Congress. The candidates sought office on party platforms offering the electorate certain policy alternatives. If a new President has been chosen, and especially if he belongs to what has heretofore been the opposition, it is inevitable that there will be new policies and new people to implement them. All or most of the top administrative positions will be vacated and refilled. This is right and necessary. But there are ways of making the transition at once speedier and more effective. And up to now there has been very little recognition of the need to do so. The Truman-Eisenhower change-over was one of the most disruptive in our history; and during the Eisenhower years no official attention has been given to the problem certain to arise in the present circumstances, which are even more perilous.

We have been living, the last months, in a situation quite as full of dangers as were imagined at the beginning of this review. It is to be hoped that when we have passed through them and are again embarked on a new administration of our affairs, the new President—or the Congress—will arrange for adequate consideration of the problem with a view to minimizing the danger to the nation in future interregnums. When this effort is undertaken Henry will be thanked for his preliminary survey of past problems and his hints for future reforms.

R. G. TUGWELL

119 Northway Road, Greenbelt, Maryland

This Is the American Earth. Ansel Adams and Nancy Newhall. Sierra Club, San Francisco, Calif., 1960. xviii + 89 pp. Illus. $15.

Any child can snap the shutter and hold a mirror up to nature, but great photography is more than luck, or even craftsmanship. Those extra ingredients of art, the artist's deep emotion and his controlled ability to communicate it, are present in many of the pictures that make up this beautiful book. Ansel Adams, whose own work makes up more than half of this collection, is a master who wields the camera's mindless eye exactly as a painter does his brush. The result is pure esthetic delight, a "stirring book," as David Brower calls it in his introduction, a fitting celebration of the glories of our national parks, and a splendid tribute to the farsighted citizens who fought to preserve them.

If this were a commercial publisher's bid for a share of the Christmas trade, one need say no more about it. The book would be a handsome gift for anyone. Far from being intended as an eye-catching gift package, however, it has a Message, promulgated by the Sierra Club, a leading conservationist organization. The message is grafted onto the photographs in the form of a text by Nancy Newhall. At the risk of seeming to throw spitballs at Home and Mother, I have to say that what comes through to me is not Conservation, but Conservationism. By this I mean a tendency to effusive overstatement that makes some conservationists sound like members of a cult.

Ordinarily the tendency is harmless enough, perhaps; but ridicule is a dangerous political weapon in the hands of one's opponents. Conservation, as a political movement, stands for intelligent, ecologically sound use of man's environment, with due regard for ethical and esthetic as well as economic standards. The preservation of wilderness is one of the essential steps in the wise use of resources, for many reasons, but scenery is not the only resource. If the proponents of preservation can be made to look ridiculous or, worse, can be charged with cant, the whole political program suffers. And it sometimes appears that we are asked to set aside more wilderness, not because it is essential, but so that more voices crying there can go unheard, except by the elect.

Nancy Newhall is a writer, not a conservationist, and cannot be held responsible for all the excesses of conservationism. For a professional writer, however, she is remarkably insensitive to language; from her wide (and probably hasty) reading she has picked up virtually all the more pretentious cliches of the cultist fringe. The consequence, if not exactly cant (it is too obviously well-intentioned for that), is a pseudopoem, so full of Message that there is no room for poetry. Even more surprising is its disharmony with the photographs: its mood is relentlessly epic where lyrics would seem to have been called for.

Fortunately, any purchaser who can get past the book's repellent title will probably not even notice the text, and the pictures *are* magnificent.

EDWARD S. DEEVEY
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Foundations of Modern Analysis. J. Dieudonné. Academic Press, New York, 1960. xiv + 361 pp. Illus. $10.

Dieudonné is a member of the famous group of mathematicians called N. Bourbaki, a group which is very formal, pure, and abstract. This book displays these traits in a humanly palatable form; however, the reader is assumed to be familiar with "advanced calculus" and an abstract variety of "elementary algebra." The purpose of the book, in the author's words, is "(a) to provide the necessary elementary background for all branches of modern mathematics involving 'analysis'; (b) to train the student in the use of the most fundamental mathematical tool of our time—the axiomatic method (with which he will have had very little contact, if any at all, during his undergraduate years)."

The author states: "It will be very apparent to the reader that we have everywhere emphasized the conceptual aspect of every notion, rather than its computational aspect, which was the main concern of classical analysis; this is true not only of the text, but also of most of the problems." He then speaks of "the necessity of a strict adherence to axiomatic methods, with no appeal whatsoever to 'geometric intuition,' at least in the formal proofs: a necessity which we have emphasized by deliberately abstaining from introducing any diagram in the book. My opinion is that the graduate student of today must, as soon as possible, get a thorough training in this abstract and axiomatic way of thinking, if he is ever to understand what is currently going on in mathematical research. This volume aims to help the student to build up this 'intuition of the abstract' which is so essential in the mind of a modern mathematician." This statement must sound very mystical to the uninitiated; its source is the fact (proved in this century) that all of the ideas of mathematics can be developed formally without any reference to the "real world." This fact sets the stage for the mathematician to act as a disembodied spirit; time and space no longer play any part in his deliberations; any intuitions he has must be of the abstract. That such intuition is possible (?) is an amazing ability of *homo sapiens;* of course, man's ability to create mathematical ideas is derived from the use of several (if not all) human faculties.

In the following interesting passage from the book, it is worth noting that words which refer to time and space are enclosed in quotation marks: "The student should as soon as possible become familiar with the idea that a function f is a single object, which may itself 'vary' and is in general to be thought of as a 'point' in a large 'functional space'; indeed, it may be said that one of the differences between the classical and the modern concepts of Analysis is that, in classical mathematics, when one writes $f(x)$, f is visualized as 'fixed' and x as 'variable,' whereas nowadays both f and x are considered as 'variables' (and sometimes it is x which is fixed, and f which becomes the 'varying' object)."

I recommend this book to those who want to be professional mathematicians and who already have had some experience with abstract methods; others enter at their own risk.

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Algenkunde. Bohuslav Fott. Fischer, Jena, 1959. vii + 482 pp. Illus. DM. 48.90.

Algae, in the broadest definition of the term, are the subject of this treatise, which is based on the author's *Sinice a Řasy,* written in Czech and published in 1956. The bulk of the book is de-

voted to a taxonomic survey, including Cyanophyta, Chrysophyta, Phaeophyta, Rhodophyta, Chlorophyta, Euglenophyta, Pyrrophyta, Cryptophyceae, Chloromonadophyceae, and colorless flagellates of uncertain position. There are brief accounts of the cytology, morphology, reproduction, and life histories of each group, followed by a systematic analysis to the level of representative genera. The last quarter of the book comprises an extensive and well-written review of algal ecology and a shorter chapter dealing with such interesting economic aspects as the significance of algae in fisheries and water-quality studies.

Those who are acquainted with Fott's careful research on microscopic freshwater algae have good reason to expect *Algenkunde* to be outstanding, but they may be disappointed. Any book that is intended to cover so vast a field in so few pages is bound to be seriously limited in its usefulness and reliability. The disproportionate allocation of space among the various divisions of algae reflects Fott's personal interests and experience, and the notable strength of the sections on Chrysophyta and Chlorophyta is balanced, unfortunately, by the weakness of the sections on other groups. The marine algae, for the most part, have not been updated beyond Oltmanns (1922–23). There is not sufficient information about individual types of algae to enable *Algenkunde* to serve as a textbook. On the other hand, the treatment is so superficial and the documentation so fragmentary that for English-speaking workers, at least, it offers no competition to Fritsch (1935, 1945) as a general reference. Curiously, the bibliographies are grossly incomplete: a check of literature citations in the text on Chlorophyta, for example, revealed that approximately half were not listed in the bibliography.

Despite these shortcomings, *Algenkunde* should prove useful to specialists and advanced students.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed not to Science, *but to the publisher or agency sponsoring the publication.)*

Admission Requirements of American Medical Colleges (Including Canada). 1960–61. Helen Hofer Gee and E. Shepley Nourse, Eds. Assoc. of American Medical Colleges, Evanston, Ill., 1960. 241 pp. $2.

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The Comparative Pharmacology of Some Psychotropic Drugs. Erik Jacobsen. World Health Organization, Geneva, Switzerland, 1960. 82 pp. Reprinted from *Bulletin of the World Health Organization* **21**, 411.

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Modern Foreign Languages in the Elementary School. Teaching techniques. Elizabeth Keesee. U.S. Office of Education, Washington, D.C., 1960 (order from Supt. of Documents, GPO, Washington 25). 65 pp. $0.45.

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Notas de Fisica. vol. 6. No. 2, "Inversion operations in quantum field theory," J. L. Lopes, 121 pp.; No. 3, "On the finite dimensionality of every irreducible unitary representation of a compact group," Leopoldo Nachbin, 6 pp.; No. 4, "Angular distribution of the reaction $^{9}Be(^{6}Li, ^{7}Li)$ ^{8}Be," L. Marquez and C. Redon, 10 pp. Centro Brasileiro de Pesquisas Físicas, Rio de Janeiro, Brazil, 1960.

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Reports

Limitations of the Competitive Exclusion Principle

Abstract. Under severe conditions in the arctic the physical environment frequently overrides biological competition, allowing essentially random occurrence of plants without distinct associations and with the frequent coexistence of related species that have extremely similar requirements. Furthermore, a mixed population may be so advantageous in reducing disease incidence as to offset competition.

Biologists are indebted to Cole (*1*) for his warning against blind acceptance of the principle that two species which occupy the same ecological niche cannot coexist, and of the dangerous corollary that two species must have different ecological requirements because they do coexist.

Any botanist who has done much field work in unfavorable environments must have doubted the universal applicability of this principle, for in very poor habitats competition often seems to be of minor importance. As a result of much of my field work in recent years having been in alpine and arctic regions, I have become increasingly impressed by the inadequacy of the exclusion principle in many habitats. Cole reminds us that Skellam (*2*) has shown mathematically that in a poor habitat greater fertility may outweigh competitive ability. Reading his report just after returning from the bleakest part of the Canadian arctic archipelago (Ellef Ringnes and adjacent islands), I could fully appreciate his argument. In this region, and to a slightly lesser degree in many other nonmountainous parts of the archipelago, the plants are severely limited in both stature and variety by the short season, very low summer temperature, high winds, and low summer rainfall. A closed plant cover is seldom approached except in the relatively scarce marshes, and on many clay or gravel plains the cover may be from 1 to 10 percent of the ground surface. Under such severe physical restrictions biological competition is greatly reduced and is often negligible. In warmer arid lands a sparse ground cover is often accompanied by an extensive root system; and the ground may thus be nearly fully occupied. But in these arctic deserts the root systems generally occupy approximately the same area as the aerial parts of the plants.

In view of the suggestion by Hutchinson (*3*) that Skellam's model applies primarily to annual plants, it should be noted that very few annuals reach the arctic, where they tend to be eliminated by occasional disastrous summers, and that only perennials occur in the high arctic.

Two features of the arctic flora emphasize the reduced importance of competition. First, there is the absence of clearly defined associations in the poorer habitats. The plants that are able to survive in such habitats generally occur randomly in any combination. This point was vividly brought to my attention on a recent arctic field trip, when a botanist unfamiliar with the arctic sought my help in identifying plant associations on arid limestone gravel. The "association" changed from step to step without any regular pattern. In such regions it is advisable to describe the flora in terms of major habitats rather than by associations.

The second significant feature of the high arctic flora is the frequent coexistence in a single habitat of two or more closely related species. When the plants are severely stunted, such mixed occurrences may be exasperating to the collector, who may later find he has mixed collections of *Draba*, *Cerastium*, or *Potentilla* spp., when he had intended to take a long series of one species. The most striking examples of such coexistence occur in *Saxifraga*, the principal genus of the high arctic. Of eight species of *Saxifraga* at Isachsen, Ellef Ringnes Island, seven occurred in strongly overlapping to essentially identical habitats; and two to five species were often found intimately mixed on a small area of uniform habitat. Here there was no problem of identity, save for the occasional simulation of *S. tenuis* by *S. nivalis*; but I doubt whether the situation is fundamentally different from some situations in temperate regions, where we have wondered whether two plants were really distinct species rather than one being a sporadic mutant of the other, because we so often find them growing together. We surely cannot draw a sharp line between severe conditions in which competition is ineffective and benign situations in which it is omnipotent in excluding a species.

A further limitation of the competitive exclusion principle is probably to be found in the effects of diseases and pests upon a solid stand of a single species. Even closely related species with identical ecological requirements often differ significantly in their resistance to diseases. A mixed population may accordingly have a decided advantage over a pure stand of one component of the mixture. This point has been well emphasized by Elton (*4*), especially in relation to agriculture, but has perhaps been inadequately appreciated by many biologists (*5*).

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23 September 1960

Protactinium-231 Content of Ocean Water and Sediments

Abstract. By means of a direct method for determining the nuclide protactinium-231, a deficiency in ocean water was found to be accompanied by unsupported Pa^{231} in ocean sediments. Protactinium-ionium ratios obtained for a surface and a deep section in the same equatorial core yielded apparent ages which were in agreement with predicted ages.

In the past, workers (*1*) set maximum concentration limits for Pa^{231} in ocean water by determining an upper limit for Th^{227} and then assuming that Th^{227}, Ac^{227} and Pa^{231} were in secular equilibrium. Since the half-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

life of 27.7 years gives ample time for solvation and precipitation processes to act selectively on Ac^{227} and effect a separation of it from its parent Pa^{231}, I thought that a direct determination of Pa^{231} in ocean water was necessary.

The procedure, a modification of one developed by Potratz and Bonner (*2*), was as follows. A known activity of Pa^{233} (a beta emitter which results from the decay of Th^{233} produced by the thermal neutron irradiation of Th^{232}), Fe^{3+} solution, and acid were added to portions of ocean water filtered through Whatman 41H paper. Naturally occurring Pa^{231} and the added Pa^{233} were concentrated by co-precipitation with $Fe(OH)_3$ upon the addition of NH_4OH and then separated from the iron by two co-precipitations on $ZrO(IO_3)_2$ from 4*M* HNO_3 solutions. Further purification of the protactinium isotopes was obtained by their extraction from a 6*M* HCl solution into diisobutylcarbinol. After washing the organic phase with several portions of 6*M* HCl, protactinium was back-extracted into a dilute HF solution which was washed with several portions of fresh diisobutylcarbinol and evaporated to dryness on a platinum plate. After these operations, the yield of protactinium was determined by comparing the Pa^{233} beta activity with that of an aliquot of the original tracer solution mounted and counted in the same manner. Counts of the total alpha activity gave a measure of Pa^{231}, since this is the only naturally occurring alpha-emitting isotope of protactinium.

For sediments, 2-g samples were digested with H_2SO_4 and addition of HF was repeated until no silica remained. After the last traces of HF had been removed by fuming with H_2SO_4, the solution was cooled and diluted, and the procedure described above was followed, starting with the precipitation of $Fe(OH)_3$.

In order to test the validity of the foregoing methods, several Pa^{231} determinations were made on material which had a known amount of uranium and the equilibrium quantity of daughter elements. The results are listed in Table 1. There is rather good agreement between experimental and theoretical values, and the experimental alpha activity did not change with time—a finding which indicated the absence of short-lived radioactive species or long-lived species with short-lived daughter elements.

On the assumption that the foregoing procedures are valid, determinations were made on two 76-lit. samples of Pacific Ocean water collected from the end of the pier of the Scripps Institution of Oceanography at La Jolla, Calif., and the results in Table 2 were obtained.

It appears that one may safely state that the Pa^{231} alpha activity in 76 lit. of ocean water is less than 0.2 disintegration/min, which is less than 3 percent of the amount which could be in equilibrium with a uranium content of 3 μg/lit. (*3*).

The deficiency of Pa^{231} in ocean water indicates that Pa^{231} is removed as it is formed from the radioactive decay of U^{235} in a time much shorter than its half-life; this behavior is similar to that of ionium (Th^{230}). Therefore, as in the case of ionium, one may expect to find quantities of unsupported protactinium in ocean sediments. This was experimentally verified for the six samples listed in Table 3.

The data indicate that there is unsupported Pa^{231} in the top layers of equatorial Pacific deep-sea sediments and that there is also the expected decrease with depth. Apparent protactinium-ionium ages for the two sections for which ionium values were obtained agree with estimates based on productivity studies and extrapolated C^{14} ages (*4*). Protactinium-ionium dating promises to be a valuable tool in the geochronological study of deep-sea sediments (*6*).

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Table 1. Protactinium-231 content of New Brunswick Laboratories AEC counter calibration sample, which contained 0.5 percent of uranium.

Sample size (g)	Yield (%)	Pa^{231}(disintegration/min)	
		Experimental	Theoretical
0.200	96 ± 4	31.6 ± 1.3	33.8
0.200	75 ± 3	28.3 ± 1.1	33.8
0.400	66 ± 2	65.0 ± 2.0	67.6
0.400	80 ± 2	64.5 ± 2.0	67.6

Table 2. Protactinium-231 content of ocean water.

Sample	Tracer yield	Total alpha activity corrected for yield and geometry (disintegration/min)
Reagent blank	72 ± 3	0.22 ± 0.06
No. 1, 76 lit.	57 ± 2	0.22 ± 0.08
No. 2, 76 lit.	36 ± 1	0.24 ± 0.10

Table 3. Protactinium content of deep-sea sediments. Ppm, parts per million; *A*, activity.

Core No.	$CaCO_3$* (%)	Total uranium (ppm)	Pa^{231} uranium equivalents† (ppm)	Th^{230} uranium equivalents (ppm)	$A_{Th^{230}}/A_{Pa^{231}}$	Apparent age (yr)
Scripps						
Capricorn 33HG 1 cm	86		342 ± 17			
Capricorn 42HG 0 cm	85		447 ± 33			
Capricorn 50HG 0 cm	76		123 ± 9			
Swedish deep-sea expedition						
Core 61, surface	65	1.5	216 ± 11	86 ± 9	9 ± 1	(0 ± 10) × 1000
Core 61, 161 cm	87		133 ± 30			
Core 61, 232–240 cm	67	1.0	86 ± 9	111 ± 11	28 ± 4	(95 ± 13) × 1000

* Cores with a high calcium carbonate content and thus with a comparatively high rate of deposition were selected for analysis in order to minimize the effect of mixing by organisms at the sediment surface on the resolution of time (*4*, *5*). Protactinium and ionium values are listed on a carbonate-free basis. † Uranium equivalents are defined as that concentration of uranium which is necessary to support the protactinium or ionium activity found, or both.

14 July 1960

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6. This is a Scripps Institution of Oceanography contribution, new series. The research was carried out under contract with the U.S. Atomic Energy Commission [contract No. AT (11-1)-34, project 44], whose support is gratefully acknowledged. I am indebted to Dr. Gustaf Arrhenius for making this work possible through his invaluable advice and encouragement, and I also acknowledge the personal communications of Dr. Edward D. Goldberg and Dr. Hans Korkisch regarding the ionium and the uranium analyses, respectively. Some of the equipment and materials were kindly furnished by Dr. James R. Arnold and Dr. William H. Thomas, and I wish to express my gratitude for this help.
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Some Effects of Ionizing Radiation on Translocation in Plants

Abstract. Petioles and apical regions of *Phaseolus vulgaris* var. Black Valentine were subjected to ionizing radiation to study the effect on the translocation process. Petiole irradiation produced no discernible effect. Inhibition of translocation to the irradiated meristems was reversed by application of the auxin naphthaleneacetic acid.

The tissue generally accepted as the locus of translocation of organic material and minerals from leaves is the phloem. More specifically, the cytological evidence would implicate the sieve

tube elements in the major role of connector between regions of supply and those of utilization ("sinks"). Tissue autoradiography showing a concentration of activity in companion cells (*1*) has cast some doubt on the validity of this conclusion. Mechanical and thermal damage to the vascular system results in reduced phloem-limited translocation; application of metabolic inhibitors indicates the process to be metabolically dependent in the phloem itself. These methods, however, do not differentiate between enucleated sieve tube elements and the nucleated cells of the phloem.

In view of the report (*2*) that nucleoplasm is more radiosensitive than cytoplasm, we felt that application of x- or β-radiation (*3*) to the phloem tissue might differentially affect the enucleated sieve tube elements and the nucleated cells of the phloem. For this study, bean plants, *Phaseolus vulgaris* var. Black Valentine, were cultivated hydroponically in a controlled-environment room. Translocation was estimated by direct counting of dried stem sections with a Geiger-Müller tube and conventional scaler after application of phosphorus-32 or of a dried ethanol extract after application of carbon-14.

Phosphorus-32 was applied as phosphate HCl (about *p*H 3) as a drop to the upper side of the leaf; C^{14} was supplied as CO_2 to the under side of the leaf by a leaf cup (*4*). The supply leaf petioles of five groups of two bean plants each were x-irradiated in doses of from 1000 to 50,000 r. Four days after irradiation, 3 μc of P^{32} were applied to the treated leaf, and after a 6.5-hour migration period, the amount and distribution of the P^{32} was determined. All differences between these plants and two control plants could be accounted for by random variation, indicating that treatment at these dosages had no effect. One bean petiole was subjected to irradiation of 683,000 reb (roentgen-equivalent-beta) over a 5.75-hour period with no apparent reduction in translocation of P^{32}. Estimates indicated that phloem tissue absorbs approximately 2 percent of the x-ray energy and 5 percent of the β-ray energy.

The radioresistance of the translocatory pathway seems to be further circumstantial evidence pointing to the enucleate sieve tube as this pathway.

Skok (*5*) has suggested that translocation of carbohydrates into "sink" regions such as meristems is directly dependent upon the cellular activity of the region. Thus a metabolically active site would reduce the substrate concentration, establish a concentration gradient, and promote translocation into the region. Support was given to this suggestion when excision of the terminal buds of sunflower resulted in an approximate 50-percent reduction in upward translocation of C^{14} from the supply leaf without significantly affecting the amount moving downward. Subsequently, Crafts and Yamaguchi (*6*) reported that phloem translocation of herbicides was correlated with both sink activity (root growth) and source activity (amount of green in variegated leaves). If we suppose Skok's hypothesis to be valid, factors directly affecting the metabolism of the meristem would indirectly control translocation into the region.

Table 1. Effect of naphthaleneacetic acid (NAA) on translocation of C^{14} to apical regions. The results are averages for three plants.

Treatment	Count/min mg (dry weight)	
	Small leaf	Meristem
Control	350.1	272.6
9900 reb	62.0	72.7
9900 reb with NAA added to meristem	91.6	212.6

Exposure of meristems to relatively small doses of ionizing radiation results in temporary reduction in growth, presumably through a reduction in auxin and deoxyribonucleic acid concentrations (*7*). To determine whether the reported growth reduction was accompanied by concomitant reduction in translocation, the apical regions of eight bean plants were irradiated with a strontium-90 source at surface incident doses (two plants at each dose) of 0.1, 1, 10, and 100 kr, respectively. Phosphorus-32 was applied 48 hours after irradiation. After a 7-hour translocation period, the amount of activity in the terminal region of the plants treated with 10 and 100 kr was less than that in the control plants by 97 and 98 percent, respectively. Findings for the other treatment groups, were not significantly different from those for the control group, indicating a threshold within the 1- to 10-kr range.

Topical application of auxin can maintain normal growth after irradiation. No natural recovery has been reported after x-ray doses above about 2 kr (*7*). If the translocation to the apical meristem region is a function of growth, topical application of auxin to the irradiated bud should also maintain translocation.

Table 1 shows the results of an experiment on three groups of three plants each. The apical region for the two irradiated groups consisted of the meristem and a small trifoliate leaf. Immediately after irradiation and again 19 hours later, one of these groups received 10 μl of a 5-parts-per-million naphthaleneacetic acid solution applied to the meristem only. Carbon-14 dioxide was released to a fully expanded primary leaf approximately 21 hours after irradiation, and the experiment was terminated 3 hours later. Irradiation at this level significantly reduced carbon translocation to the irradiated portions; however, application of auxin did result in the translocation of significantly more activity to the meristem than to the irradiated leaf.

While there is no real way of differentiating causes and effects in this case, the known relationship between auxin and growth and the correlation between auxin and translocation demonstrated in this study clearly support the hypothesis that translocation is correlated with and probably dependent upon cellular or metabolic activity at the site of utilization (*8*).

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8. This report is contribution No. 642 of the department of botany and plant pathology, Ohio State University.

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22 August 1960

Transplantation and Malignancy in a Companion-Host System on the Chorioallantoic Membrane

Abstract. Pairs of tissue fragments in contact with one another were transplanted onto the chorioallantois. Ten days after transplantation, embryonic lung and liver were found to be accepted, without obvious infiltration of one another. When Walker tumor was substituted for either of the normal tissues, the pair of transplants was also accepted by the chorioallantois, and in addition there was extensive invasive replacement of the normal tissue by the cancer transplant.

In the search for the essence of cancer in a microsystem, reference is often made to bioassay methods for evidence that the elemental factor under study is related to some quality of

malignancy. Growth of a suspected tissue, transplanted in a conditioned host, has been used most extensively as an assay. The degree to which successful transplantation in a treated heterologous host can be equated with malignancy is uncertain, since many normal embryonic tissues and benign tumors will "take" in heterologous hosts while many highly malignant clinical cancers will not (*1*). Furthermore, the conditioned host has been considered an improper test object, even though the "take" exhibits all the microscopic features considered to be diagnostic of cancer by the pathologist (*2*).

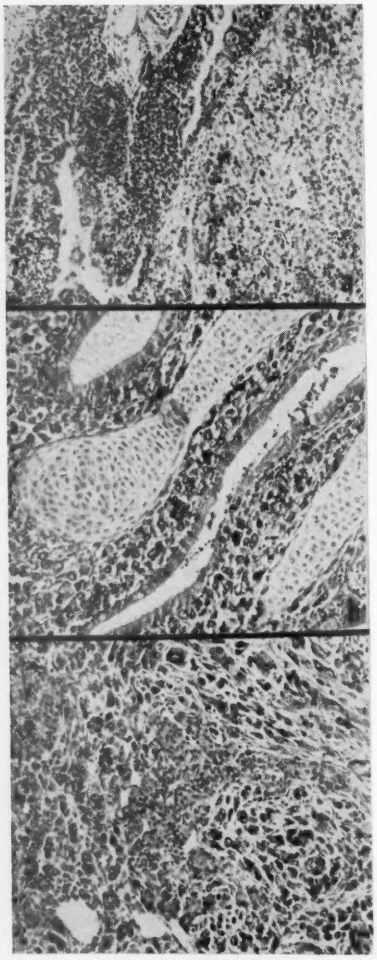

Fig. 1. Comparison of the interactions between three different pairs of transplants after 10 days on the chorioallantois (hematoxylin and eosin stain, about × 75). *a*, Lung (left) and liver (right). The integrity of each tissue is preserved. There is no invasive replacement of one tissue by the other. *b*, Lung and tumor. Except for bronchial epithelium and cartilage, the lung parenchyma has been completely replaced by tumor cells. *c*, Liver and tumor. The liver tissue (identified in the center of the field as acinar groups of cells with small nuclei) is invaded and extensively replaced by tumor cells.

In the studies described here we used a *companion host system* (*3*). These studies were undertaken to distinguish between phenomena that may be purely evidences of successful transplantation and others that clearly represent malignancy, that is, invasive replacement of normal parenchymal tissues (*4*). The tissues involved were three that are readily transplanted to the chorioallantoic membrane of the chick: chick embryonic liver and lung and the Walker carcinosarcoma 256 of the rat (*5*). Paired explants of these tissues were placed side by side on the chorioallantoic membrane as follows: liver plus lung, Walker tumor plus lung, and Walker tumor plus liver.

Transplants consisted of pieces 1 or 2 mm in diameter of Walker tumor tissue from subcutaneous passage in the rat, and of liver and lung from 15- or 16-day-old chick embryos. Pairs of explants were placed touching one another on the freshly dropped chorioallantoic membrane of 8- or 9-day-old recipients. In some series (see Table 1) the fragments were cemented together with a chick plasma clot. All the eggs were opened on the 18th day of incubation, 9 or 10 days after inoculation. The site of transplantation was identified, and semiserial sections were prepared of each.

On gross examination, the pairs of inocula that included a tumor fragment produced the largest nodules, some up to 1 cm in diameter. The pairs of normal tissues produced nodules of less than 5 mm in diameter.

On microscopic examination of combinations that included tumor, the tumor was seen to occupy two-thirds or more of the total mass of the transplant. Masses of tumor cells infiltrated the normal tissues extensively, and in many instances only focal residua of the normal tissue could be identified (Fig. 1). In three instances invasion was not observed although both transplants were identified. In these cases, either the inocula were separated or else massive necrosis and inflammation made positive identification of interaction impossible.

In the combinations of normal liver and lung, invasion or destruction of one tissue by the other was not observed. In the three instances listed with a plus-minus in the "invasion" column of Table 1, occasional glandular units of the liver and epithelial structures of the lung shared a common stromal bed, but replacement of one by the other was not suggested.

Under the conditions of our experiments, two qualities can be distinguished, transplantability and invasion. The latter is generally considered to be an essential feature of malignancy. The two transplanted embryonic tissues when presented as a pair do not suggest neoplasia. They retain their orderly architecture. Furthermore no replacement, destruction, or invasion of one tissue by the other is seen. When the Walker tumor is substituted for either normal partner, it not only "takes" in an aggressive fashion in the chorioallantoic membrane, but it invades and replaces the normal tissue with which it is in contact.

Some might argue that the observations described here are properly interpreted as simply the effect of the greater growth potential of the Walker tumor on the chorioallantoic membrane. We think that this kind of explanation contributes very little to an understanding of the mechanisms involved. Growth need not be infiltrative, but can be expansive with compression of normal parenchyma. The capacity for rapid growth of the Walker tumor is probably a necessary factor if invasion is to occur, but rapid growth does not in itself account for the invasive capacity of the tumor. It does not explain, for example, why cartilage and bronchial epithelium are spared when all other structures are replaced.

Many normal tissue "soils" can be exposed to the same tumor in companion host systems. We may be able to refine our assay of the functional capacity of a tumor in a highly pertinent context—invasive replacement of specific host tissues.

Furthermore, we hope that the two concurrent biologic events, transplantation and malignancy, occurring at the same site may be individually modified

Table 1. Data compa-ing interaction of tissues on the chorioallantoic membrane. In all series but 6 and 9 the explants were cemented together with chick plasma prior to completion of the inoculation.

Series No.	Membranes (No.)		
	No. examined	Both transplanted tissues identified	Invasion seen
	Liver and lung		
1*	12	7	(1 ±)
2†	11	8	(1 ±)
3*	7	6	0
4†	20	19	(2 ±)
	Walker tumor and lung		
5*	12	9	8
6†	9	8	8
7*	13	10	10
	Walker tumor and liver		
8*	12	9	8
9†	9	9	8
10*	11	8	8

* Eight-day-old chorioallantoic membrane inoculated with 15-day-old organ fragment.
† Nine-day-old chorioallantoic membrane inoculated with 16-day-old organ fragment.

with pharmacologic agents. Perhaps lung or liver can be changed from being a good soil for invasive tumor spread to a poor soil without important impairment of other normal attributes.

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5. Furnished by Dr. Bernard Fisher, University of Pittsburgh.

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15 August 1960

Relationship between Reaction Time and Electroencephalographic Alpha Phase

Abstract. Demonstrations of a relationship between human 8 to 13 per second (alpha) electroencephalographic activity and simple visual reaction time can be made at reliable levels of confidence by (i) sampling reaction times to stimuli given at phases of the alpha cycle 10 msec apart, (ii) selecting the phase with the slowest reaction times, and (iii) collecting enough reaction times to stimuli at this and some other control phase for statistical comparison.

Several investigators (*1*) have speculated on the possibility of a parallel between phasic changes in behavior and the 8 to 13 per second activity of the electroencephalogram, known as the alpha rhythm. Lansing (*2*) compared reaction times to stimuli which coincided with six different portions of the alpha cycle. He found no reliable overall differences, and to achieve statistical significance he was forced to select retrospectively the slowest and fastest groups of reaction times for comparison. He also screened 100 subjects in order to find eight with enough alpha activity for his procedure.

Our paper describes a technique that (i) allows identification of that portion of the alpha cycle most likely to be associated with slow reaction times in advance of statistical evaluation, and (ii) is applicable to about a third of an unselected adult population by virtue of requiring only moderately dominant alpha activity.

The individual being tested is seated with eyes closed in a darkened room. Alpha activity is lead off from the left occipital area and either the left ear lobe or the mid-frontal scalp. This is amplified and fed into a circuit designed to generate an electrical signal only when the phase and amplitude of the alpha rhythm correspond with a predetermined setting. This initial signal is followed by a second signal after a delay that can be varied manually from 10 to 90 msec.

The second signal flashes a strong light in the subject's face and simultaneously starts a timer. The subject responds to this light by pressing a key; his reaction stops the timer, automatically prints the reaction time, and furnishes a voltage analog of reaction time to the Y axis of an automatic X-Y plotter. By supplying the X axis of this plotter with a voltage corresponding to the delay between the two signals, one can plot each reaction time as a function of approximate alpha phase at stimulation.

If an artifact never triggered the first signal of this device, and if alpha waves were perfect sine waves of fixed frequency, then the delay between the first and second signals would specify exactly the alpha phase at the instant of visual stimulation. However, neither of these two ideals is realized. Therefore, a sweep on a monitor oscilloscope is started by the first signal and this displays the electroencephalographic (EEG) activity for 150 msec. In this way the subject's EEG activity can be visualized at the instant of each stimulation. Stimuli that fail to coincide with the desired portion of an alpha cycle are discarded from further consideration. In spite of this check, however, there is possible error of as much as 15 msec for individual stimuli.

Using this device, one can present blocks of 10 to 20 stimuli at steps of 10 msec each along the alpha cycle, and in this way accumulate a scattergraph of reaction time as a function of approximate alpha phase at stimulation. One can then select the alpha phase at which stimuli elicit the slowest responses and compare this to one other phase—either a phase associated with fast responses or one about 180 degrees away. The results of selecting phases in this way are illustrated in Table 1. Data in this figure were collected after the initial screening runs by presenting 10 to 15 stimuli at a time at one phase. Rest periods were introduced after each 20 to 40 stimuli, and the order of phase presentation was alternated to minimize the effects of learning and fatigue on differences between reaction times to stimulation at the two selected phases.

These data represent only reaction times measured after a prediction; therefore, one-tailed tests of significance could be justified. In this case, six out of the eight subjects would be found to yield significant results at the 5 percent level or better. However, to test the over-all group, two-tailed probabilities are given in the Table. Combining these by the chi-square formula, we find that differences in reaction times to stimuli presented at different alpha phases in eight independent studies such as this would have occurred by chance about one time in 500.

At the onset, we elected to disregard reaction times over 300 msec as probably representing lapses of attention. The number of reaction times disregarded in this way are also given in Table 1. This procedure biased the data against the obtained results and significantly more reaction times were excluded from the slow samples than from the fast samples.

Because of individual variability, phase at stimulation is given in milliseconds from maximum occipital positivity. Average alpha cycle duration is also indicated for each subject. In this series stimuli presented nearest to maximum occipital positivity elicited slowest reaction times. This corresponds to the

Table 1. Visual reaction times to phasic stimuli. All times in milliseconds. Stimulus phase is given in milliseconds from maximum occipital positivity. $X^2 = -2\Sigma\log_e p = 38.9$, (16 df).

Subject	Predicted slow				Approx. av. alpha period	Predicted fast				Statistics	
	Stimulus phase	Reaction time				Stimulus phase	Reaction time			t	$p <$
		Under 300		Over 300 (No.)			Under 300		Over 300 (No.)		
		Mean	No.				Mean	No.			
L. H.	0	243	47	6	100	50	231	49	3	2.22	.05
D. M.	−20	240	40	3	100	−30	226	40	3	2.21	.05
I. V.	0	249	50	10	100	20	240	50	9	2.48	.02
A. W.	0	247	107	16	110	40	241	105	15	1.68	.10
I. S.	−10	262	46	20	100	40	250	51	12	2.50	.02
J. M.	−10	239	77	5	90	20	236	79	3	0.87	.40
P. P.	0	262	49	17	100	−40	252	40	9	1.80	.10
R. D.	0	215	73	9	100	40	216	80	2	0.15	.90

segment of the alpha cycle where Lansing found stimuli associated with slowest responses.

These data indicate a more than chance relationship between visual reaction time and alpha phase at stimulation. Finer details of the relationship between alpha phase and reaction time must wait further investigation.

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2 September 1960

Use of Pectinase in Preparation of Mitochondria from Tobacco-Tissue Cultures

Abstract. Mitochondrial particles were isolated from tobacco cells in tissue cultures which were disintegrated with pectinase. The pectinase treatment of the tissue cultures reduced the dry weight of the particulate preparations and eliminated glutinous material, but caused little change in oxidative and phosphorylative activities. The pectinase apparently did not affect the enzymatically active sites of the particles.

Pectinase has been used to disintegrate tobacco cells in tissue cultures for the preparation of mitochondria. The hope was that pectinase would eliminate the glutinous nature of the pellet secured from centrifuging the homogenate of the tissue cultures. Thus, uniform suspensions of mitochondria could be obtained readily in the reaction medium. This approach to enzymic disintegration of tissue seemed promising because of the successful isolation of mitochondria from pigeon heart muscle and rat liver by the use of proteinase (*1*). Prior incubation of such tissues with proteinase facilitated the grinding procedure, minimized mechanical damage to the particles, and produced particles with a higher P/O ratio, greater stability, and a greater general reactivity.

The tobacco tissue used in this experiment (*2*) was a single cell clone (H196) from a stem of *Nicotiana tabacum* × *N. glutinosa,* and was grown on a modified White's agar medium (*3*). Mitochondria were prepared with pectinase as follows: 150 g of tissue, 2 weeks old, was washed by suspending it in 400 ml of 0.5*M* mannitol, and was separated from the mannitol solution by filtration on two layers of cheesecloth. The washed cells then were suspended in 400 ml of 0.5*M* mannitol to which 600 mg of pectinase (Nutritional Biochemicals Corp.) was added; the suspension was kept at 2°C for 45 min with gentle stirring with a magnetic stirrer. The suspension became more acidic with time and was adjusted to *p*H 7.2 with 0.5*M* potassium hydroxide as needed. After incubation, the suspension was homogenized in a glass homogenizer with a Teflon pestle. During grinding, the *p*H was again adjusted with potassium hydroxide. The pectinase-treated tissue was soft and was readily ground by three in-and-out movements of the pestle in the mortar as compared to five in-and-out movements with nontreated tissue suspension. The particles were separated and washed by centrifugation (*4, 5*). Oxygen consumption was measured with an oxygen electrode apparatus (*6*), and phosphate was determined by King's method (*7*).

Table 1 shows typical results of analyses of mitochondria prepared from pectinase-treated tissue as compared to mitochondria prepared from nontreated tissue. The yield of mitochondria on the basis of wet and dry weights was decreased to less than half by the pectinase treatment. At the same time, the glutinous material in the original preparation was removed, and the particles could be suspended easily in the reaction medium. The protein content in particulate preparations was little affected by the pectinase treatment; the ratio of nitrogen to dry weight was almost doubled by the treatment.

The particulate preparations from both pectinase-treated and nontreated tissues oxidized succinate, citrate, glutamate, α-ketoglutarate, reduced diphosphopyridine nucleotide (DPNH) and ascorbate. For comparison, oxidation rates for succinate, DPNH, and ascorbate are shown in Table 1. The pectinase treatment apparently caused no significant change in oxidative activities and in the relative activities of the enzymes. The P/O ratio also was unchanged by the treatment. Essentially the same results were obtained with mitochondria originally isolated from nontreated tissue but incubated with pectinase (1 mg of pectinase per milliliter of mitochondrial suspension) for 15 minutes at 2°C after isolation (see column 9, Table 1). After incubation with pectinase, the wet weight of the mitochondrial preparation decreased to less than half, and the cohesive nature of the original preparation disappeared. The oxidative and phosphorylative activities of mitochondria treated with pectinase were comparable to those of the particles prepared from either pectinase-treated or nontreated tissue cultures. Mitochondria isolated from pectinase-treated tissue cultures or mitochondria treated with pectinase after isolation responded more to adenosine diphosphate (ADP) than mitochondria isolated from nontreated tissue.

The pectinase treatment seemed to eliminate polysaccharide substances which otherwise might contaminate the pellet of mitochondria and cause them to aggregate tenaciously. This treatment,

Table 1. Comparison of mitochondrial preparations from pectinase-treated and nontreated tissue cultures of tobacco and a mitochondrial preparation treated with pectinase after isolation.

Item	Mitochondria from				Mitochondria* treated with pectinase after isolation	
	Nontreated tissue		Pectinase-treated tissue			
Wet weight	2.36 g		0.91 g		0.79†	
Dry weight	209 mg		91.5 mg			
Protein–N	2.52 mg		2.24 mg			
Protein–N/dry weight	0.0125		0.0243			
(Oxidative activities)	(mμatoms/min per mg N)	(Relative activity)	(mμatoms/min per mg N)	(Relative activity)	(mμatoms/min per mg N)	(Relative activity)
Succinate oxidation	18.7	1.0	14.1	1.0	16.6	1.0
Succinate oxidation in presence of ADP	105	5.6	102	7.3	133	8.0
DPNH oxidation	342	18.3	293	20.8	293	17.7
Ascorbate oxidation	1790	95.7	1500	106.1	1540	92.8
P/O ratio with succinate	0.85–1.0		0.84–1.0		0.87–1.0	

* Mitochondria originally isolated from nontreated tissue cultures. † Wet weight before pectinase treatment was 1.79 g.

however, apparently did not affect the enzymatically active sites of the particles, since there were only slight changes in oxidative and phosphorylative capacities. From the little change in the ascorbic acid oxidase activity of the preparations with and without pectinase treatment, the ascorbic acid oxidase activity seemed intrinsic to the particles of tobacco-tissue cultures, as was reported for mitochondrial preparations from tomato-tissue cultures (*5*).

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10 August 1960

Isolation of Listeria monocytogenes from Oat Silage

Abstract. A culture of *Listeria monocytogenes* isolated from the brain of an infected sheep from a flock fed contaminated silage and cultures isolated from this silage were all serological type 4b, establishing a definite epidemiological relationship.

In spite of considerable effort by a number of investigators, the epidemiology and pathogenesis of all forms of infection due to *Listeria monocytogenes* remain obscure (*1*, *2*). Of the more than 700 confirmed cases of listeric infection in man (*1*) and the several thousand in animals, only in a few instances has it been possible to trace the source of infection. An apparent relationship between silage feeding and listeric infection in ruminants has been mentioned often during the two decades since Olafson (*3*) first suggested it. In Iceland this relationship is so striking that listeriosis is known as *votheysveiki*, or silage sickness (*4*). Recently Gray (*5*) reported the isolation of *L. monocytogenes* from the viscera of two out of 20 mice fed corn silage thought to have been responsible for an outbreak of listeric abortion in range cattle. Serological typing of the cultures isolated from the mice and from the aborted bovine fetuses revealed that all were type 1 (*1*), strongly suggesting an epidemiological relationship.

Early in March 1960 oat silage was given as supplementary feeding to a flock of 900 sheep in northwestern Montana. About 1 week later 16 sheep showed symptoms suggestive of listeric encephalitis and some died. The brain of one sheep killed in the terminal stage of the disease was cultured, and *L. monocytogenes* was isolated from it. Unfortunately, only one other sheep brain was submitted for culture. This sheep died considerably later, and all attempts to isolate the bacterium from it failed.

Silage from the same area of the pit as the silage which had been fed to the sheep was divided into several portions. One of these was fed to 15 mice. Although all died within 14 days, there were no gross lesions suggestive of listeric infection at necropsy, and the bacterium was not isolated from any of the mice, even though suspensions of the liver and spleen were refrigerated for longer than 4 months (*6*).

Extracts of four other portions of silage taken at random from the sample submitted were prepared by suspending approximately 25 g of silage in 100 ml of sterile distilled water. These extracts were held at 4°C for at least 48 hours. Five mice were then inoculated intraperitoneally with 0.5 ml of each extract. This was repeated with three extracts after a 1-month interval. Generally, within 24 hours after exposure some mice in each group were lethargic and a few appeared acutely ill. Of 35 mice exposed, 32 died. First deaths usually occurred on the 2nd postexposure day, and only a few mice survived more than 4 days. Necropsy usually revealed moderate to severe focal hepatic necrosis characteristic of listeric septicemia. Even though a few dead mice showed no gross lesions, *Listeria monocytogenes* was isolated in essentially pure culture from a liver and spleen pool of every mouse that died. Three mice in one group remained asymptomatic and were killed on the 8th postexposure day. In spite of their healthy appearance, two of these had a few necrotic foci on the liver, and the bacterium was isolated from all three.

The four silage extracts also were plated on tryptose agar (Difco). The plates were incubated for 24 hours at 37°C and examined for the presence of characteristic blue-green colonies of *L. monocytogenes* with a binocular scanning microscope and obliquely transmitted light (*6*). In general, it was not possible to detect colonies of *L. monocytogenes* in extracts that had been refrigerated less than 10 days, as they were often rather heavily contaminated by a variety of bacteria and molds. Cultures prepared after 10 days of refrigeration usually revealed from five to 15 or more colonies of *L. monocytogenes*. The bacterium was isolated in this manner from all the extracts prepared.

The fact that all cultures isolated during this study were type 4b (*1*) strongly suggests that the sheep actually were infected by ingestion of the contaminated silage and establishes an important link in the epidemiology of listeric infection in ruminants.

These findings indicate not only that this silage contained rather large numbers of *Listeria monocytogenes* but also that it was of sufficient pathogenicity to incite active infection both in the naturally exposed sheep and in artificially exposed mice. Further proof of its pathogenicity was the rapid production of a marked purulent conjunctivitis and keratitis after instillation of the culture into the conjunctival sac of rabbits.

These findings, coupled with the earlier observations of Gray (*5*) and of Gislason and Vigfusson (*7*), not only support but also offer a possible explanation for the apparent higher incidence of listeric infection among ruminants fed silage than among those fed other rations. Superficially, these findings tend to support the concept that in listeric encephalitis the bacterium enters through the digestive tract; however, this is contradicted by observations during naturally induced outbreaks of the disease, by the consistent failure to produce the disease artificially through oral exposure (*1*, *2*), and by the failure to infect mice fed this silage. The findings reported here are not necessarily at variance with the view of those who hold that listeric encephalitis results either from exposure through the upper respiratory tract or by migration of the bacterium along branches of the trigeminal nerve (*2*), since either system would be vulnerable during ingestion of a forage such as silage.

Although these results establish that silage may contain *Listeria monocytogenes* in sufficient numbers to cause infection under field conditions, they give no clue to the manner in which the silage becomes contaminated. It is possible that *L. monocytogenes* is a soil bacterium or even a common inhabitant of some plant life (*1*). Also, silage could be contaminated by ground game or birds, which are known to be potential carriers of the bacterium (*1*, *2*).

This investigation reemphasizes the importance of refrigeration of material to be cultured for the presence of *L.*

monocytogenes and stresses the inadequacy of present isolation techniques in which a nonliving culture medium is used.

Studies identical with those described above have been carried out on silage from other areas where listeric infection developed shortly after the initiation of silage feeding, but the bacterium has not been isolated from any of these, to date (*8*).

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7. After the results of this study were made known to Dr. Palsson of the University of Iceland, he wrote that in 1945 in their laboratory Dr. Gislason and Mr. Vigfusson had isolated *L. monocytogenes* from guinea pigs inoculated with silage extracts and that the bacterium had been isolated directly from silage on one occasion since that time. Regretably, these important findings were never published.
8. The technical assistance of Mrs. Jean Martin is gratefully acknowledged. This work was supported in part by research grant No. E-2571 from the National Institute of Allergy and Infectious Diseases, U.S. Public Health Service. This report is a contribution (paper No. 515, journal series) from the Montana Veterinary Research Laboratory (Montana Experiment Station and Livestock Sanitary Board cooperating), Montana State College, Agricultural Experiment Station.

15 August 1960

Thermal Annealment and Nitric Oxide Effects on Free Radicals in X-irradiated Cells

Abstract. Four kinds of radicals are identified in dry spores (*Bacillus megaterium*) after x-irradiation: those associated with singlet, doublet, triplet, and oxygen-complex types of spectra. The singlet, present only at low temperatures, gives rise irreversibly to the doublet at 25°C. All hyperfine structure is depressed after annealment at 100°C and is lost when oxygen or nitric oxide is added. The physical results support the hypothesis that reactions of long-lived free radicals can account for the radiobiological phenomena of thermorestoration, nitric oxide protection, and latent oxygen effect.

Studies by Powers *et al.* (*1*) on the influence of physical and chemical factors on the response of nonmetabolizing dry spores to x-irradiation, as measured by colony-forming capacity, have revealed a systematic sensitivity to such factors as gas and temperature not only during but also after irradiation. These results suggest several classes of damage, one of which is thought to be due to long-lived free radicals. Upon reaction with oxygen these radicals may form lethal complexes; alternatively, they may be rendered harmless if scavenged by chemical recombination or annealed by thermal energy. This report presents physical evidence for the existence of such long-lived radicals with the postulated properties in irradiated dry spores.

The electron spin resonance spectrometer was a conventional microwave system (9.35 kMcy/sec) with a reflection cavity; a double modulation technique was used. The resonance patterns are approximately the second derivative of the absorption signal (*2*). The basic theory and practice of electron spin resonance spectroscopy have been treated by Ingram (*3*). Lyophilized bacterial spores were irradiated in 25-mg lots in a beryllium-window exposure chamber in an atmosphere of helium gas, at a temperature of −195°C. The x-ray source was equipped with a wolfram target and a beryllium window, operated at 50 kv and 30 ma (constant potential), at a dose rate of 50 kr/min. After irradiation the spores were quickly dumped into another section of the chamber, composed of a quartz tip (protected from radiation), shaped like the spectrometer cavity. The tip was then transferred into the cavity (also at —195°C). The spectra shown in this study were obtained after irradiation at doses of 4000 kr; essentially similar but quantitatively weaker signals were obtained at doses as low as 250 kr. No signals were found with unirradiated spores.

The initial spectrum (no warm-up) is shown at upper left in Fig. 1*A*. The spores were then warmed up to 25°C for specified intervals and returned to the cavity for measurement at −195°C. On warming over a period of 22 minutes, a singlet component of the initial spectrum appears to be converted into a doublet with 20-gauss spacing. A triplet present in the initial spectrum remains unchanged. Growth of the doublet at the expense of the singlet at 25°C is shown in Fig. 1*B*. the five-line spectrum at 22 minutes (Fig. 1*A*), consisting of a 1:2:1 triplet with 30-gauss spacings and the 1:1 doublet superimposed, is essentially the same as that seen when spores are irradiated at room temperature (25°C, Fig. 2*a*).

In viability studies, exposure of the spores to higher temperatures after anoxic irradiation results in thermorestoration of a considerable fraction (*4*). The physical results are similar in that a reduction of the number of radicals (electron spin resonance) by as much as 50 percent is effected by annealment at 100°C for 10 minutes (Fig. 2*c*). Furthermore, in the viability studies, if the anaerobically irradiated spores are exposed to oxygen before annealment, no part of the potential damage is reversible. In these studies, if the anaerobically irradiated sample is first exposed to oxygen at 25°C, the spectra shown in Fig. 2*b* result (dotted line, 1.5 minutes; solid line, after 3 minutes). This represents a new oxygen-complex radical resulting from the reaction of radicals *R2* and *R3* (Fig. 1*A*) with oxygen. The residual radicals in the annealed system (Fig. 2*c*) also react with oxygen to form a typical oxyradical spectrum (Fig. 2*d*) but of reduced strength. This is in general agreement with the biological results.

Again, the discovery from the viability studies that nitric oxide enhanced the survival of anaerobically irradiated spores (*5*) led to the hypothesis that the protective action of nitric oxide could be explained by its scavenging action upon long-lived oxygen-reacting radicals. The physical results are in accord with this hypothesis in showing nearly total obliteration of the hyperfine spectra by nitric oxide (50 percent

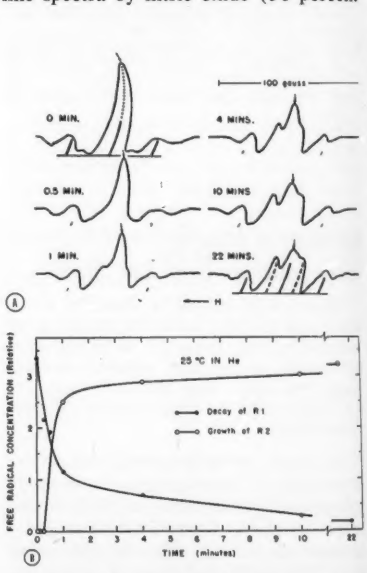

Fig. 1. (*A*) Paramagnetic resonance measured at −195°C in dry bacterial spores previously irradiated anaerobically (4×10^6 r in He at −195°C; warmed in He at 25°C). A hyperfine doublet (broken lines) appears between 1 and 22 minutes, lateral to the center line of an initial triplet (solid lines), while the superimposed initial singlet (dotted line) disappears. Arrow represents position of g (= 2.003). (*B*) Kinetics of destruction of the singlet component (*R1*) of the center line and growth of the doublet (*R2*) at 25°C anaerobically. [The amplitude of meter deflection is plotted directly for *R2*; the asymptotic values of the triplet (*R3*) center line were subtracted in computing *R1*.]

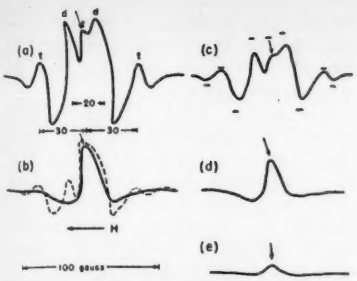

Fig. 2. Paramagnetic resonance measured at −195°C in dry bacterial spores previously irradiated anaerobically at 25°C. (*a*) Hyperfine patterns composed of doublet *d* and triplet *t* spectra. (*b*) Spectra produced by reaction of these radicals with O_2 at 25°C (see text). (*c*) Anaerobic annealment of radicals in (*a*) at 100°C for 10 minutes; signal heights for this sample before annealment are given by the horizontal lines. (*d*) Reaction with oxygen of radicals remaining after annealment. (*e*) Reaction of radicals in (*a*) with nitric oxide.

in helium) applied after irradiation (Fig. 2*e*). Similar biological and physical results with nitric oxide have been reported independently by Sparrman *et al.* (*6*) in another system.

While the close correlation shown between the biological and physical measures of radiation damage does not require any causal relationship of events, the results do support with physical evidence derived from the same system the proposed mechanism (*1*) of the latent oxygen effect (*7*).

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7. We are pleased to acknowledge valuable discussions with Dr. M. L. Randolph of Oak Ridge National Laboratory. This work was performed under the auspices of the U.S. Atomic Energy Commission. These results were announced on 26 February 1960 at the fourth annual meeting of the Biophysical Society in Philadelphia. An abstract appeared in the *Proceedings of the Biophysical Society* (1960), p. 36.

27 June 1960

Simultaneous Generalization Gradients for Appetitive and Aversive Behavior

Abstract. In the presence of a bright light five monkeys were trained to press a lever to avoid shock and to pull a chain for food reward. When tested with a series of lights dimmer than the conditioning stimulus, the monkeys showed a sharp gradient of effect for the rewarded response, in contrast to a very flat gradient for the avoidance response.

When an organism is trained to make a response in the presence of a particular stimulus, this response will also normally occur in the presence of other stimuli that are physically similar to the original conditioning stimulus. The result of this phenomenon is often a "gradient of stimulus generalization"—an orderly decline in the probability of response, which takes place as the physical difference between the original stimulus and various test stimuli is increased. Stimulus generalization is a major explanatory concept in such areas as learning theory, psychotherapy, and abnormal psychology (*1*).

The study reported here was designed to determine whether there is any difference between generalization gradients for reward-controlled and punishment-controlled behavior. The technique used to investigate this problem permitted a comparison of the two gradients for individual subjects and may be applicable to other problems in experimental psychology and psychopharmacology.

Five young male rhesus monkeys were the subjects. The experimental test chamber was a commercially produced model (Foringer) which provided an automatic mechanism for reward delivery, an electrifiable grid, implementation for two possible responses (pulling a chain which hung in the center of the chamber and pressing a lever mounted on one wall), and a 110 v a-c, 60-watt house light mounted above a circular screen of milk glass in the top of the chamber. During generalization testing the intensity of this house light was varied in discrete (though unequal) steps by means of a group of fixed resistors in series with the house light. The 11 possible test-light intensities were calibrated on several occasions with a General Electric foot-candle light intensity meter placed approximately 1 foot below the glass screen on which the house light was projected.

All the subjects were first trained to press the lever, which postponed shock for 10 seconds; by responding at least once every 10 seconds the subjects could avoid shock entirely (*2*). During this training period, and all subsequent training periods prior to the generalization test, the house light was on continuously at its maximum intensity (28.1 ft-ca).

After the animals became proficient at avoiding shocks, the chain was introduced into the apparatus, and each pull of the chain was rewarded with a pellet of food. The avoidance schedule was still in effect, so that lever-pressing continued even during the learning of the chain-pulling response. Eventually all the subjects learned to press the lever to avoid punishment and to pull the chain to produce food reward on a 2-minute variable-interval schedule (*3*).

After ten additional sessions of exposure to the concurrent schedules of reward and avoidance in the presence of the brightest light intensity (*4*), a generalization test session was programmed. During this test, light of 11 different intensities was presented, 12 times at each intensity, in a mixed order. Each stimulus presentation lasted 30 seconds. No rewards or punishments were obtainable during this test. The test procedure was quite similar to that of Guttman and Kalish (*5*).

For several experimental sessions after this generalization test the subjects were put on concurrent reward-avoidance schedules as before; they were then given a second generalization test (*6*).

Chain-pulls and lever-presses in response to each of the 11 light intensities were recorded. Generalization gradients, relating response strength (the ratio of total number of responses to each intensity to total number of responses to all intensities) to log intensity of the test light are shown in Fig. 1. These data are group means for the two generalization tests combined. Considered separately, the data for individual subjects are very similar to the group results.

Figure 1 displays a clear difference

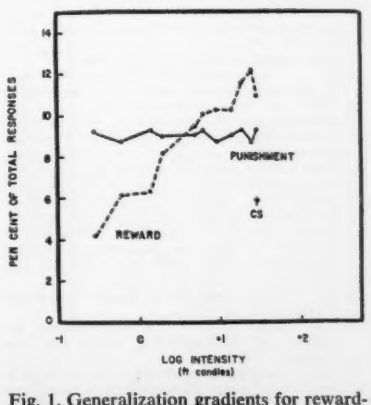

Fig. 1. Generalization gradients for reward-controlled and punishment-controlled behavior.

between avoidance and reward gradients. The reward gradient is much steeper; it was found that the subjects were all much more likely to respond to stimuli of a high intensity (close to that of the conditioned stimulus) than to stimuli of much lower intensity than the conditioned stimulus. In contrast, the avoidance gradient is almost completely flat; subjects were just as likely to respond to the dimmest as to the brightest test light (*7*).

Since the rate of avoidance responding was much higher than the rate of responding for food reward, the differences in shape of the generalization gradients might be attributable to differential response rates rather than to motivational or reinforcement factors (reward versus punishment). However, at least one similar study (*5, 8*) has shown that lowered response rate leads to a flattening of generalization gradients, a finding which would imply the opposite effect from that obtained in the experiment discussed here.

The finding of virtually indiscriminate avoidance response, in contrast to the well-discriminated rewarded response, may have relevance to clinical descriptions of hypersensitivity and seeming irrationality under conditions of strong anxiety; an "anxious" patient may respond strongly to stimuli which are only remotely similar to an original anxiety-provoking stimulus. There are experimental data from studies of human beings which also show a greater than normal amount of stimulus generalization in subjects who are highly anxious (*9*) or even schizophrenic (*10*), or who are made anxious experimentally (*11*).

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4. Note that the animals were not given any discrimination training—that is, they were not exposed to any other house-light intensities during training in the experimental chamber. One subject, however, usually had to be left in the apparatus overnight, since his daily session began at 5 P.M.; at the conclusion of the session the house light went out, and the animal remained in complete darkness until the next morning. The generalization data for this subject did not differ in any obvious way from those for the other subjects.
5. N. Guttman and H. I. Kalish, *J. Exptl. Psychol.* **51**, 79 (1956).
6. One subject contracted a digestive-tract infection and died before a second test was possible.
7. After the conclusion of this experiment subjects were given discrimination training, so that they learned to press the lever and pull the chain only during light of one intensity (the brightest or dimmest, depending on the subject) and to cease responding when the light was of a different intensity (at the other end of the intensity continuum). Here, too, preliminary results showed avoidance gradients to be flatter than reward gradients. Both gradients were much steeper than before discrimination training, however.
8. With regard to "response rate" it might be added that D. R. Thomas and R. A. King [*J. Exptl. Psychol.* **57**, 323 (1959)] and M. Sidman (Eastern Psychological Association meetings, 1960) found no effect of response strength on generalization. However, W. O. Jenkins, G. R. Pascal, and R. W. Walker [*J. Exptl. Psychol.* **56**, 274 (1958)] found significantly flatter gradients in their more active subjects.
9. See, for example, S. A. Mednick, *J. Consult. Psychol.* **21**, 491 (1957); E. R. Hilgard, L. V. Jones, S. J. Kaplan, *J. Exptl. Psychol.* **42**, 94 (1951).
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19 September 1960

Properties of the Major Component of a Peptic Digest of Rabbit Antibody

Abstract. The molecular weight of the active, major component isolated from a peptic digest of rabbit antibody was found to be 106,000. After treatment with a disulfide-splitting reagent, the molecular weight was 56,000, and the products migrated as a single peak in the ultracentrifuge. The univalent fragments thus formed can be partially recombined by passage through IR-120 cation-exchange resin at room temperature or by treatment with a difunctional organic mercurial. Some splitting of the pepsin-treated antibody molecule occurs on carboxymethylcellulose at *p*H 5.4.

Porter (*1*) has shown that papain hydrolyzes rabbit antibody into three chromatographically separable fractions, two of which block precipitation of the homologous, untreated antibody with antigen. The third is inactive but crystallizable. Fragments of rabbit antihapten antibody were found to have nearly all their specific binding sites intact (*2*) and were shown to be univalent (*3*). Peptic digestion of the antibody results in a decrease in sedimentation coefficient, for the bulk of the protein, from about 6.5 to 5 *S* (*4*). The fragments are still bivalent, as is indicated by their capacity to precipitate specifically. Subsequent treatment with one of several disulfide-splitting reagents splits the 5 *S* residue into 3.5 *S*, univalent fragments (*4*). This is accomplished by the reduction of a single, highly reactive disulfide bond (*5*). Since papain is a sulfhydryl enzyme, and is therefore used in conjunction with a disulfide-splitting reagent as activator, it was proposed (*4*) that the two enzymes may act by similar mechanisms. This suggestion was supported by the close similarity in several properties of the final products obtained by the action of either enzyme with a reducing agent present.

The method used (*5*) for isolation of the 5 *S* fragments of antibody resulting from peptic digestion consists, first, in precipitation with sodium sulfate added to a final concentration of 12.5 percent (w/v). After centrifugation, sodium sulfate is added to the supernatant to a final concentration of 19 percent. The precipitated protein thus obtained, in several preparations, migrated as a single peak with $s_{20,w} = 5.0 \pm 0.2$ *S*. The yields were 40 to 60 percent of the weight of gamma globulin used.

The molecular weight of this purified component of a peptic digest (of rabbit antiovalbumin gamma globulin) was determined. The diffusion constant was measured in a synthetic boundary cell in a Spinco model E ultracentrifuge at a protein concentration of 10 mg/ml, and sedimentation coefficients were determined at concentrations of 2.5, 4.0, 7.0, and 10 mg/ml. Both procedures were carried out at 20°C in saline-borate buffer, *p*H 8, ionic strength 0.16. The sedimentation coefficient, s_0, obtained by extrapolation to zero concentration, was 5.25 *S* and the diffusion constant was 4.7×10^{-7} cm²/sec. The partial specific volume was taken as that of untreated antibody, 0.745 (*6*), giving a molecular weight of 106,000.

After treatment of the above preparation with 0.01*M* 2-mercaptoethylamine and dialysis against saline-borate buffer, the value of s_0 at 20°C was 3.6 *S*, and the diffusion constant was 6.1×10^{-7} cm²/sec, corresponding to a molecular weight of 56,000. Symmetrical single peaks were observed for both preparations. Since the 3.6 *S* fragments migrated as a single peak, the results suggest that the reducing agent splits the molecule into two subunits approximately equal in molecular weight. This is consistent with the possibility (*4*) that the 5 *S* molecule consists of Porter's Fractions I and II, linked through a disulfide bond.

In other experiments, described below, it was found that chromatography of the purified 5 *S* material on carboxymethylcellulose at *p*H 5.4 causes partial degradation into fragments with $s \cong 3.5$, having the capacity to block the homologous precipitin reaction of untreated antibody with antigen. The results are similar to those obtained on treatment with a reducing agent.

We have also found that the 3.5 *S* fragments can be recombined to give fairly good yields of 5 *S* protein. This has been done either by passage through the ion-exchange resin, IR-120, at *p*H 5,

or by treatment with a bifunctional organic mercurial. Details of the experiments follow.

One hundred milligrams of a purified peptic digest of rabbit antiovalbumin gamma globulin ($s_{20,w}$ = 5.2) was adjusted to *p*H 5.4, ionic strength 0.007, in acetate–chloride buffer. The solution was added to a 22 by 360 mm column of carboxymethylcellulose, containing 0.7 meq of carboxyl groups per gram, obtained from the Brown Company, Berlin, N.H. Three fractions were collected by successive elutions with acetate buffers of *p*H 5.4 and ionic strengths of 0.007, 0.1, and 0.9. The fractions contained 39, 30, and 13 percent, respectively, of the total protein.

The first two fractions were concentrated by pervaporation, carried out with the lower end of the dialysis bag immersed in constantly stirred saline–borate buffer, *p*H 8, μ = 0.16, and were dialyzed against the same, cold buffer. Approximately 90 percent of the protein in the first fraction migrated with a velocity (s_{20}) of 3.2 *S*; a small amount of somewhat faster moving material was present. The second fraction exhibited two peaks with s_{20} = 3.5 (55 percent) and 5.1 (45 percent), respectively. The effect of 2 mg of either fraction on the reaction of 1.4 mg of untreated antiovalbumin gamma globulin with an optimum concentration of antigen was tested, and essentially complete inhibition was observed in each case. A heavy precipitate was formed by the antibody in the absence of inhibitor.

The next experiments, on recombination, were done with the products obtained by treatment of the purified 5 *S* protein with 0.01*M* 2-mercaptoethylamine hydrochloride for 1 hour at 37°C in 0.05*M* acetate buffer, *p*H 5.0. After the reaction, the protein migrated as a single peak with s_{20} = 3.4 *S*. This concentration of 2-mercaptoethylamine (0.01*M*) has been found to be approximately minimal for complete breakdown into ~3.5 *S* fragments under the conditions used (*5*).

Sixty milligrams of the reduced protein in 3 ml was passed through an 8 by 150 mm column of IR-120 resin in the sodium cycle at *p*H 5.0 and room temperature; 0.05*M* acetate buffer, *p*H 5.0, was used for elution. The eluate gave a negative nitroprusside test. In the ultracentrifuge 40 percent of the protein now migrated with a velocity (s_{20}) of 5.1 *S*, suggesting that the 3.5 *S* fragments had partially recombined into the original 5 *S* units. This result has been confirmed in several similar experiments.

In another experiment with 250 mg of protein and 0.01*M* 2-mercaptoethylamine in a volume of 8 ml, the reducing agent was removed on a shorter column (8 by 75 mm) in the cold room at *p*H 6. Under these conditions there was very little recombination; the nitroprusside test on the eluate again was negative. Portions of the eluate were treated at *p*H 5 with increasing amounts of the bifunctional organic mercurial, 3,6-bis-(acetoxymercuri methyl)-dioxane (*7*, *8*), added as a 0.002*M* solution in water. The molar ratios of the mercurial to 5 *S* protein originally present (molecular weight 106,000) were 0.4, 0.7, 1, and 4:1. After standing overnight in the refrigerator, a small amount of precipitate, representing less than 6 percent of the total absorbance at 280 mμ in each case, was removed by centrifugation. For the samples containing 0.4, 0.7, and 1.0 times the equivalent amount of mercurial, the extents of recombination were 45, 50, and 35 percent, respectively. The s_{20} values were 4.9, 5.0, and 4.9, respectively, for the faster peaks and 3.4, 3.3, and 3.4 for the slower peaks in the three experiments. A control, incubated overnight without the mercurial, showed no appreciable change in the sedimentation pattern (s_{20} = 3.3).

When four times the equivalent amount of mercurial was used, over 90 percent of the protein migrated with a velocity of 3.7 *S*. This failure to recombine is similar to the results observed in the dimerization of serum albumin (*7*), and can probably be attributed to the fact that excess mercurial reacts in a 1:1 rather than 1:2 ratio with the sulfhydryl (SH) groups (*7*). Actually, under the most favorable conditions quantitative recombination to 5 *S* fragments would not necessarily be expected because of the difficulty of producing univalent fragments with exactly one SH group per molecule. The stoichiometry requires further investigation. The precipitation observed may have been due to polymerization of units containing more than one SH group.

The results indicate that the univalent fragments comprising the 5 *S* protein are partially separable under the mild conditions prevailing on a column of carboxymethylcellulose at *p*H 5.4. This is probably not attributable to impurities in the adsorbent, for when fresh, untreated Whatman's coarse cellulose powder was used instead of carboxymethylcellulose, nearly half the protein was similarly degraded to 3.2–3.4 *S*. Two possible explanations are: (i) There are sufficiently strong reducing groups in the cellulose to split the S-S bond, and (ii) the large surface catalyzes a rearrangement which forces the fragments apart. Other possibilities also exist and the mechanism is at present uncertain.

The recombination on IR-120 can probably be attributed to reoxidation of sulfhydryl groups. This would be consistent with the observation that the bifunctional mercurial also brought about a large amount of recombination to units having almost exactly the same *s* value. Experiments aimed at resynthesis of precipitating antibody from blocking fragments are in progress. It should also be of interest to attempt to prepare antibody of mixed specificity (*9*).

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9. This work was supported by grant E-2858 from the National Institutes of Health.
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15 August 1960

Letters

(Continued from page 1732)

pedagogy that the teacher's knowledge of subject matter content might turn out to be far less than desirable. As I see it, the problem is one of proper balance. Some knowledge of the technique of teaching would, in my opinion, be highly desirable for the majority of college teachers.

I believe it is a vast oversimplification to state, as Helwig does, "that the two essential characteristics of a good teacher are (i) enthusiasm and (ii) thorough knowledge of and interest in his subject." I believe that most people concentrate on "teaching" and forget that the important process is really its complement "learning." This takes place within and *only* within the mind of the student. If our educators would exhibit more concern for learning, then teaching, as such, would take care of itself. I submit that the key factor in the education process is the motivation of the learner; I would venture further that the role of the teacher is primarily that of a motivator and only secondarily that of an imparter of knowledge. Actually the student may acquire knowledge from books, audio visual aids, direct experience, or other means. College teaching, as contrasted with secondary school teaching, is concerned with the development within the student of the power to think, reason, appreciate, and discriminate; but the exercise of intellect necessary to become learned requires self-discipline on the part of the student, not enthusiasm and interest of the teacher, or only insofar as it makes the student want to learn.

There appears to me only a tenuous relationship between learning on the part of the student and either enthusiasm or thorough knowledge of and interest in the subject matter on the part of the teacher. If these two attributes of the teacher serve to motivate the student, so much the better, but one should not rule out other techniques of motivation. Unfortunately one can think of a number of teachers who possess these two qualifications and yet who are not considered by either their peers or their students to be very good teachers.

We must recognize also that college teaching embraces a wide range from the instruction of beginning freshmen to the occasional contact with the mature graduate student pursuing studies in either purely academic or professional fields. It is my personal opinion that teaching the elementary college level courses makes the greater demands on the motivation skill of the teacher; whereas, in graduate work, motivation may be more readily derived from the teacher who is both enthusiastic and possessed of great knowledge, because both of these qualities provide high motivation to the advanced student.

It is in this latter situation particularly that I fear that any state licensure might only serve to eliminate many of our greatest and most widely influential teachers.

MERRITT A. WILLIAMSON
College of Engineering and Architecture, Pennsylvania State University, University Park

Left-handed Subjects

I am engaged in research requiring left-handed subjects of all mating patterns. We particularly need sinistral individuals at least 16 years of age with both parents left-handed.

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SIDNEY WEINSTEIN
Albert Einstein College of Medicine, Yeshiva University, New York

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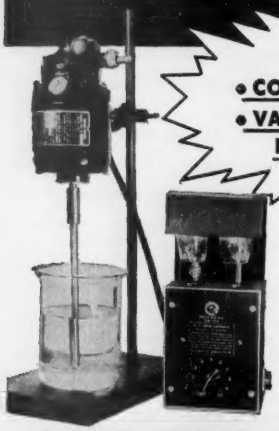

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The baron led the way

The great Baron Berzelius may well have sensed big game when he discovered pyruvic acid in 1835. The name comes from the Greek *pyr*, for fire, and the Latin *uva*, for grape. This figures. Pyruvic acid used to be prepared by dry distillation of tartaric acid. The oldtimers had it spotted as an intermediate in the fermentation of sugar. Such relationships had probably begun to look simple and straightforward.

It was just as well that no gypsy fortuneteller told the Baron that 72 years after his death work would begin in earnest on unraveling *just* how grape sugar breaks down into pyruvic acid and that this work would take 40 years until the phenomenon was at last understood as the first stage in the process by which chemists and other living creatures obtain from their food the energy with which to engage in all affairs, including ratiocination about biochemistry.

Anyway, the Baron was an excellent chemist. He prepared several salts of pyruvic acid, including the lithium. Now we wish to report that we, too, can prepare *Pyruvic Acid Lithium Salt* and, in fact, offer it as Eastman 8130, a standard for use in pyruvic acid determinations. *Pyruvic Acid* is Eastman 498, vintage unspecified.

Of very recent vintage is the new Eastman Organic Chemicals, List No. 42, which catalogs some 3800 organic compounds we stock. If you do not have a copy, why not? Write Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).

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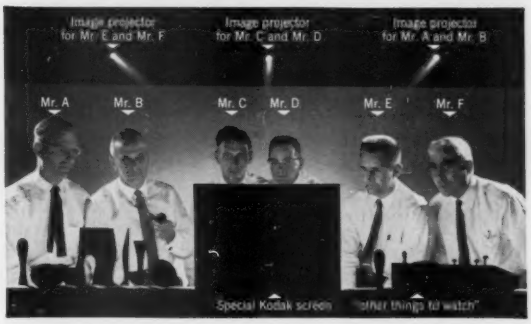

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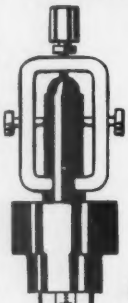

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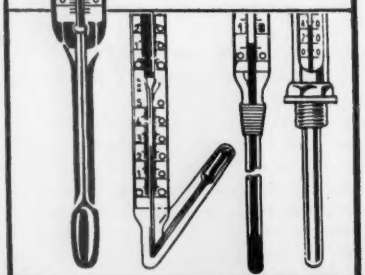

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Meetings

Migration of Marine Organisms

A Latin-American symposium on migration of marine organisms was held in Guayaquil, Ecuador, from 27 June to 1 July. It was organized jointly by the UNESCO Science Cooperation Office for Latin America and the University of Guayaquil.

It was the fifth of the series of meetings on marine sciences which the office has held since its foundation in 1949. The previous meetings, like the Guayaquil meeting, had the double character of symposium and working-party session. They were held at Concepción, Chile, in 1954; São Paulo, Brazil, in 1955; Montemar, Chile, in 1956; and Montevideo, Uruguay, in 1957.

Enrique Rioja (Mexico) was elected chairman of the Guayaquil symposium, and Hugo Ferrando (Uruguay) acted as secretary. Thirteen marine scientists, from the following countries, participated: Argentina, Brazil, Chile, Cuba, Ecuador, Mexico, Peru, and Uruguay. The UNESCO Science Cooperation Office for Latin America was represented by Angel Establier.

The program of the meeting included sessions on the following subjects: (i) migration of marine organisms (fish; invertebrates; cephalopods; birds; other vertebrates); (ii) environmental factors (oceanographic and biological) that cause migration; (iii) standardization of methods for studying migratory phenomena, with a view to comparing results; (iv) establishment of regional programs for work on migration of aquatic populations; and (v) other problems related to migration of marine organisms.

At the conclusion of the symposium, the following recommendations were unanimously approved by the participants.

1) The symposium, considering that the study of the migration of marine organisms by its very nature requires a joint effort on the part of the countries concerned, recommends regional action. The following delimitation of problems common to a number of countries is made with a view to their being undertaken regionally. Argentina, Brazil, and Uruguay: Thunnidae, Scombridae, Clupeidae (*Sardinella* sp.), Galeorhinidae (*Galeorhinus vitaminicus*), and Mugilidae (*Mugil* sp.). Chile and Argentina: Merluccidae (*Merluccius* sp.), Decapoda (Macrura: "langostinos" shrimps, and "langosta" de Juan Fernández). Peru and Chile: Thunnidae (*Neothunnus macropterus*), Engraulidae (*Engraulis* sp.), Scombroidae (*Sarda* sp.), and Decapoda (*Munida* sp.). Peru, Ecuador, and Colombia: Thunnidae (*Katsuwonus* sp.; *Neothunnus macropterus*), Decapoda (Macrura: "langostinos"). Mexico, Caribbean, and coast of Venezuela: Thunnidae (*Katsuwonus* sp.; *Thunnus* sp.), Centropomidae (*Centropomus* sp.), Decapoda (Macrura: *Panulirus* sp.; Penaeidae: *Penaeus* sp.).

2) It is recommended that information be exchanged between Argentina and Chile covering research results and methods used in work on the principal species of *Merluccius*.

3) Marking trials should be held in conjunction with studies on the biological cycle of the species (growth, reproduction, alimentation, and so on), with a view to relating migration and the different phases of life of individuals. Similarly, the importance of the study of environmental conditions emphasized at the meeting of the UNESCO Marine Sciences Working Party, held at Montevideo 22 to 24 May 1957, is underlined.

4) In view of the importance of movement and mixture of different bodies of water, it is expressly recommended that Argentina, Brazil, and Uruguay should study the northern and southern limits and fluctuations, respectively, of subantarctic and subtropical waters and also the origin of the water of the coastal region. It is hoped that similar work may be undertaken for the southeast Pacific area in the fairly near future.

5) UNESCO is asked to interest the governments of Latin America in supporting this type of scientific work, which is an indispensable basis for a proper knowledge of the renewable resources of the sea.

6) It is recommended that the next marine sciences meeting to be organized by the UNESCO Science Cooperation Office for Latin America should be divided into two sections: "Abiotic oceanography" and "Marine biology."

J. SWARBRICK
Centro de Cooperacion Cientifica Para America Latina, Montevideo, Uruguay

Forthcoming Events

December

26–31. American Assoc. for the Advancement of Science, annual, New York, N.Y. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5)

The following 52 meetings are being held in conjunction with the AAAS annual meeting.

AAAS Committee on Science and the Promotion of Human Welfare (B. Commoner, Shaw School of Botany, Washington Univ., St. Louis 5, Mo.). 26, 28, 29 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics (J. R. Mayor, Director of Education, AAAS, Washington, D.C.). 28, 29 Dec.

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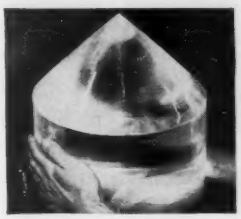

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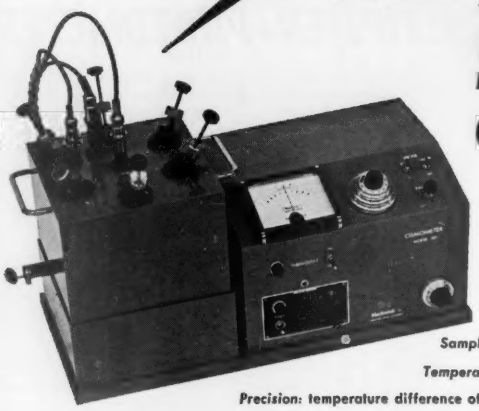

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AAAS Symposium Volume No. 63

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Allen D. Bass and Gordon K. Moe, Editors June 1960

Presented at the AAAS Washington meeting, December 1958.

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American Assoc. of Clinical Chemists (H. Goldenberg, Dept. of Biochemistry, Hillside Hospital, P.O. Box 38, Glen Oaks, N.Y.). 26–27 Dec.

American Assoc. of Scientific Workers (Miss M. Yevick, 214 Western Way, Princeton, N.J.). 27 Dec.

American Astronautical Soc. (R. Fleisig, 58 Kilburn Rd., Garden City, N.Y.). 27 Dec.

American Astronomical Soc. (J. A. Hynek, Dearborn Observatory, Northwestern Univ., Evanston, Ill.). 28–31 Dec.

American Council on Women in Science (Miss E. B. Thurmann, Div. of Research Grants, National Insts. of Health, Bethesda 14, Md.). 27 Dec.

American Economic Assoc. (K. E. Boulding, Dept. of Economics, Univ. of Michigan, Ann Arbor). 26 Dec.

American Geophysical Union (R. Jastrow, NASA Theoretical Div., 8719 Colesville Rd., Silver Spring, Md.). 26 Dec.

American Nature Study Soc. (R. E. Hopson, 4138 S.W. Fourth Ave., Portland 1, Ore.). 27–30 Dec.

American Psychiatric Assoc. (P. H. Knapp, Boston Univ. School of Medicine, Boston, Mass.). 29, 30 Dec.

American Soc. of Criminology (D. E. J. MacNamara, New York Inst. of Criminology, 115–117 W. 42 St., New York 36). 26, 27 Dec.

American Soc. of Naturalists (R. C. Rollins, Gray Herbarium, Harvard Univ., 22 Divinity Ave., Cambridge 38, Mass.). 27 Dec.

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American Sociological Assoc. (V. H. Whitney, Dept. of Sociology, Wharton School of Finance, Univ. of Pennsylvania, Philadelphia, Pa.). 28, 29 Dec.

American Statistical Assoc. (R. E. Lewis, New York Area Chapter, 55 Wall St., New York 15). 29 Dec.

Association of American Geographers (C. Morrison, Jr., American Geographical Soc., Broadway at 156 St., New York 32). 27–30 Dec.

Association for Computing Machinery (W. F. Cahill, NASA, 8719 Colesville Rd., Silver Spring, Md.). 29 Dec.

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Biomedical Information Processing Organization (R. S. Ledley, Natl. Biomedical Research Foundation, Silver Spring, Md.). 30 Dec.

Committee on Cosmetics, American Medical Assoc. (J. B. Jerome, 535 N. Dearborn St., Chicago 10, Ill.). 29 Dec.

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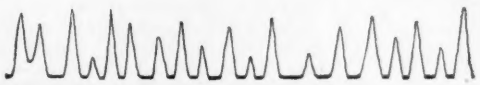

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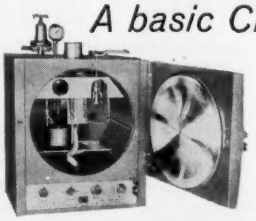

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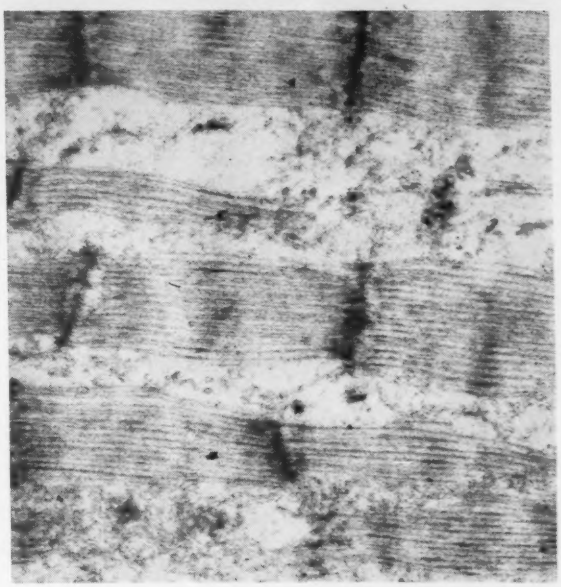

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National Assoc. for Research in Science Teaching (G. Mallinson, School of Graduate Studies, Western Michigan Univ., Kalamazoo). 27 Dec.

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Science Clubs of America (Miss L. V. Watkins, Science Service, 1719 N Street, NW, Washington 6). 30 Dec.

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27–28. Council for the Improvement of Scientific Communication—Cong. on Educology and Role Growth Theory of Communication and Learning, New York, N.Y. (R. L. Switzen, 840 Grand Concourse, New York 51.)

27–29. Conference on Strong Interactions, Berkeley, Calif. (A. C. Helmholz, Dept. of Physics, Univ. of California, Berkeley.)

27–29. Northwest Scientific Assoc. and Idaho Acad. of Science, joint meeting, Moscow. (E. J. Larrison, Dept. of Biological Sciences, Univ. of Idaho, Moscow.)

27–14. Bahamas Surgical Conf., Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

28. Association for Education in International Business, St. Louis, Mo. (J. N. Behrman, Univ. of Delaware, Newark, Delaware)

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(See issue of 18 November for comprehensive list)

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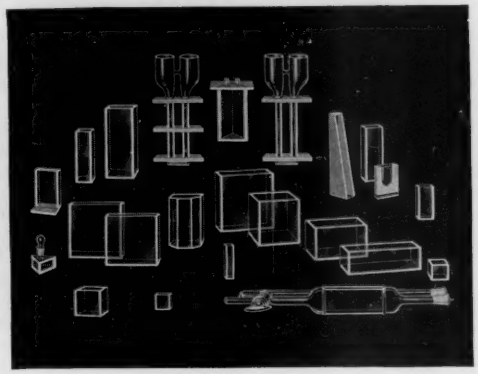

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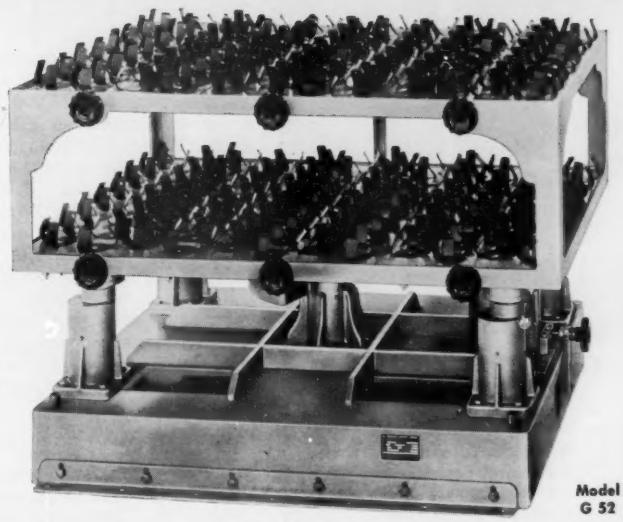

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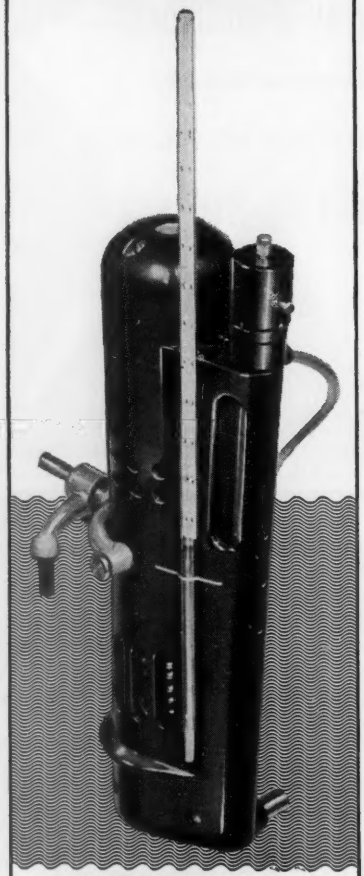

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New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ HIGH-VACUUM PUMPING STATION switches automatically from roughing setting to high-vacuum setting upon reaching a predetermined pressure. The 4-in. system is said to achieve pump-down to 5×10^{-6} mm-Hg in less than 35 sec and ultimate pressure less than 5×10^{-7} mm-Hg. Manual control is also provided. (Veeco Vacuum Corp., Dept. Sci951, Terminal Drive, Plainview, N.Y.)

■ TURBIDIMETER of Wagner type determines the fineness of Portland cement. Included is all apparatus needed in the test as specified by ASTM. Included are photoelectric cell, approximately collimated light source, timing burette and stand, microammeter, wet screening assembly, sedimentation tanks, flasks, test tubes, and a stirring apparatus. (Soiltest, Inc., Dept. Sci953, 4711 W. North Ave., Chicago 39, Ill.)

■ THERMOCOUPLE for measurement of surface temperature is said to have a response time less than 10 μsec. Thin-ribbon thermocouple wire and mica insulation 0.002 in. thick are used. Sensing tip extension up to 4 in. permits access through thick container walls. Operating temperature range for continuous service is 2000°F and operating pressure ranges to 25,000 lb/in.^{2} Models are available in all standard thermocouple elements. (Nanmac Corp., Dept. Sci954, P.O. Box 8, Indian Head, Md.)

■ PROXIMITY SWITCH responds to any metal part that remains within range of the pickup for 50 msec or more. Input is 117 volts, 50 to 60 cy/sec at 15 watts; output is a single-pole, double-throw relay rated at 5 amp, noninductive. A sensitivity control permits adjustment of operating range. Size is 6 by 8 by 4 in. (Electro Products Laboratories, Inc., Dept. Sci961, 4500 N. Ravenswood Ave., Chicago 40, Ill.)

■ DECADE COUNTER element provides digital display of 1½ in. numerals, zero through nine, said to be readable at 30 ft. Counting rate is 100 kcy/sec. Power requirements are 12 volts d-c at 240 ma and 20 volts d-c at 10 ma. Triggering pulse may be positive or negative (4 volts minimum, 12 volts maximum). Output impedance is 330 ohms. The elements, measuring 2⅜ by 1½ by 8½ in., can be combined as a preset counter. (Telemetrics, Inc., Dept. Sci962, 12927 S. Budlong Ave., Gardena, Calif.)

■ PROGRAMMED CALIBRATION SUPPLY delivers preselected calibration voltages over the range 0 to 10 volts from a constant-impedance output of 10 ohms. Absolute accuracy is said to be better than ±0.01 percent for line voltage variations of ±10 percent over the temperature range from 40° to 125°F; tracking accuracy better than ±0.005 percent; and 30-day stability better than ±0.005 percent. Selection of 10 programmed voltages is provided by mercury contactors. (Owen Laboratories, Inc., Dept. Sci955, 55 Beacon Place Pasadena, Calif.)

■ EPOXY ADHESIVES are offered in small packages for mixing and using in small quantities without waste. The packages are small plastic containers each holding a premetered quantity of adhesive and catalyst. Types offered are metal-to-metal adhesive, glass-to-ceramic bonding adhesive and Teflon treating agent. (Plastic Associates, Dept. Sci964, 2900 S. Coast Ave., Laguna Beach, Calif.)

■ BEARING ANALYZER, for nondestructive inspection of ball and roller bearings, operates on the basis of vibrations produced by the rotating bearing. Indication is provided by a meter, a cathode-ray-tube display, and a loudspeaker. Average test time is said to be less than 30 sec. Bearings from ⅛ in. to 10 in. outside diameter are accommodated. (Bearing Inspection, Inc., Dept. Sci965, 3311 E. Gage Ave., Huntington Park, Calif.)

■ GROUND DETECTOR senses the presence of a ground in isolated a-c systems used in anesthetizing areas. Both visible and audible warnings are given. The detector is housed in a single ventilated enclosure that may be located for best access. Units are said to conform to the 60,000 ohm capacitive reactance specification recommended by the National Electrical Code. (Custom Control & Manufacturing Co., Dept. Sci958, P.O. Box 291, Buena Park, Calif.)

■ LOW-NOISE AMPLIFIER has a nominal band width of 0.1 cy to 320 kcy/sec, adjustable in steps. Gain is adjustable in 3-db steps from 100 to 10,000. Noise developed with input shorted is less than that developed across a 1000-ohm resistor at room temperature. Noise at full band width is less than 3 μv; it drops to 1.5 μv at 80 kcy/sec band width. Hum is less than 0.25 μv, distortion less than 0.5 percent. Maximum output is 30 volts (peak). Output impedance is less than 100 ohms. (Electronic Instruments, Dept. Sci957, 1130 Mission St., South Pasadena, Calif.)

JOSHUA STERN

National Bureau of Standards, Washington, D.C.

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A symposium presented at the AAAS Washington Meeting, December 1958. Published June 1960.

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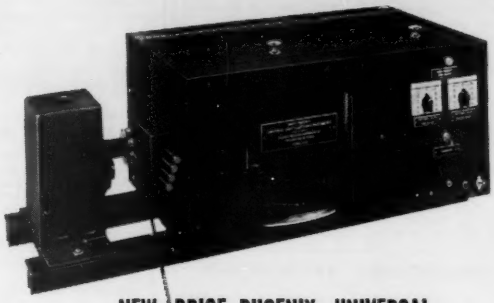

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1. The two-session AAAS General Symposium, "Moving Frontiers of Science V"—Speakers: Edward Anders, H. W. Magoun, George Wald, and H. H. Goldstine; Thomas Park, presiding.
2. The "Challenge to Science" evening with Sir Charles P. Snow, Theodore M. Hesburgh, and W. O. Baker; Warren Weaver, presiding.
3. On "AAAS Day," the three broad, interdisciplinary symposia—Plasma: Fourth State of Matter; Life under Extreme Conditions; and Urban Renewal and Development, arranged by AAAS Sections jointly.
4. The Special Sessions: AAAS Presidential Address and Reception; Joint Address of Sigma Xi and Phi Beta Kappa by Polykarp Kusch; the Tau Beta Pi Address; National Geographic Society Illustrated Lecture; and the first George Sarton Memorial Address by René Dubos.
5. The programs of all 18 AAAS Sections (specialized symposia and contributed papers).
6. The programs of the national meetings of the American Astronomical Society, American Nature Study Society, American Society of Zoologists, History of Science Society, National Association of Biology Teachers, Scientific Research Society of America, Sigma Delta Epsilon, Society for General Systems Research, Society for the Study of Evolution, Society for the History of Technology, Society of Systematic Zoology, and the Society of the Sigma Xi.
7. The multi-sessioned special programs of the American Association of Clinical Chemists, American Astronautical Society, American Geophysical Union, American Physiological Society, American Psychiatric Association, American Society of Criminology, Association of American Geographers, Ecological Society of America, Mycological Society of America, National Science Teachers Association, New York Academy of Sciences—and still others, a total of some 90 participating organizations.
8. The four-session program of the Conference on Scientific Communication: The Sciences in Communist China, cosponsored by the AAAS, NSF, and ten societies.
9. The sessions of the Academy Conference, the Conference on Scientific Manpower, and the conference of the American Council on Women in Science.
10. The sessions of the AAAS Cooperative Committee on the Teaching of Science and Mathematics, and of the AAAS Committee on Science in the Promotion of Human Welfare.
11. Titles of the latest foreign and domestic scientific films to be shown in the AAAS Science Theatre.
12. Exhibitors in the 1960 Annual Exposition of Science and Industry—103 booths—and descriptions of their exhibits.

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